VOL. 20, NO. 4 December, 1945

THE QUARTERLY REVIEW of BIOLOGY

THE PROBLEM OF CENTRAL NERVOUS REORGANIZATION AFTER NERVE REGENERATION AND MUSCLE TRANSPOSITION

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INTRODUCTION

IF THE regeneration of nerve fibers after peripheral nerve lesions regularly restored in perfect detail the original connections between the central nervous system and its end-organs, nerve regeneration would not present a problem of central nervous reorganization. To attain such perfect regeneration it would be necessary that each one of the hundreds of fibers of various types in the proximal nerve stump be guided into its own original channel in the distal stump and hence out to its proper end-organ in the periphery. However, such perfect guidance of regenerating fibers back to their appropriate terminations is, of course, never achieved or even approximated after nerve section. Misdirection of the outgrowing fibers, resulting in their ioss and in their haphazard redistribution to foreign end-organs, has long been recognized as a conspicuous feature of nerve regeneration (Langley, 1918; Cajal, 1928; Weiss, 1937a; Young, 1942). If normal smooth function is to be restored to the reinnervated parts, extensive central nervous readjustments must therefore take place.

The following list of factors all tending to cause distortion of the normal innervation pattern following nerve lesions indicates the nature and extent of the readjustment problems with which the centers must cope. (1) The outgrowth of fibers from the proximal nerve stump and their entrance into the neurilemmal tubes of the distal stump is apparently non-selective, with no biochemical attractions or affinities of individual fibers for their original pathways. The motor fibers of a flexor muscle, for example, may become scattered fortuitously into various foreign channels leading to extensor, pronator, supinator or any other skeletal muscles or autonomic effectors supplied by the severed nerve. Motor fibers may also be misdirected into sensory channels. As far as is known, any regenerating fiber of a severed nerve may enter and grow distally within neurilemmal tubes previously occupied by any other fiber, or between such tubes. (2) The direction of outgrowth of fibers across the gap between the nerve stumps is dependent upon submicroscopic mechanical factors which are very easily disturbed, causing deflection of the fibers from a straight course. Assuming that the nerve ends have been neatly apposed in correct orientation, any of the following or similar factors may cause gross deflection of fibers into foreign pathways: (a) local foci of infection; (b) irritative foreign bodies, such as suture materials; (c) scar formation; (d) irregular tension and pressures, or simply absence of straight longitudinal tension on the substrate between the nerve ends. (3) Regenerating fibers show a tendency to undergo multiple branching in the scar region, so that single fibers become connected with several end-organs, which may all be of different function. (4) With the common methods of suture many fibers become lost in local neuromas, in Perroncito coils, and in scar tissue around the point of union. Also, a varying percentage of fibers fail to enter tubes of the distal stump and instead grow distally between the tubes. Such inter-tubal fibers must often become lost or take an abnormal course in the periphery. (5) Regenerating nerve fibers are of much smaller diameter than mature fibers, with the result that as many as ten or more regenerating fibers often enter a single tube formerly occupied by only one axon, leaving pathways to other end-organs completely uninnervated. (6) The extreme microscopic precision required in the apposition and orientation of the nerve ends to assure an accurate spanning of the nerve gap is only a hypothetical ideal which remains utterly unattainable in practice. Recent advancements in nerve splicing methods (Weiss, 1943a, b) promise to reduce the multiple branching, loss of fibers, and tortuous outgrowth across the nerve gap; but even if perfectly straight outgrowth across the gap could be obtained, it would result in normal innervation only if the cross sections of the two nerve stumps were aligned with microscopic exactness. The best methods of nerve splicing when applied under optimal conditions can be expected to yield at most only a statistical predominance of appropriate over inappropriate nerve fiber connections. (7) In clinical surgery, conditions frequently make it impossible even to attempt any accuracy in orienting and aligning the cut faces of the nerve stumps. Accidental lesions are rarely clean transections which permit neat reunion of the nerve ends. In delayed sutures, scar formation often obscures beyond recognition the original orientation of the nerve stumps, leaving their alignment at secondary apposition largely a matter of chance. Resection of fibrotic nerve ends is usually necessary in such cases. If intraneural plexuses (Langley and Hashimoto, 1917) are present in the excised stretch of nerve, then accurate apposition even of fascicles will be impossible, even though the general orientation of the nerve may still be discernible. (8) Frequently the gap between the nerve ends is so large that direct reunion of the nerve with the limb in normal position is impossible. Recent evidence (Highet and Sanders, 1943; Weiss and Taylor, 1943a; Weiss, 1943c) indicates that the use of grafts to bridge such large gaps may be more favorable for nerve regeneration than the extreme manipulative measures favored in the past. The presence of grafts or other types of bridge spanning the gap must necessarily greatly enhance abnormal redistribution of regenerating fibers. Internal nerve plexuses in the nerve graft or in the excised portion of the nerve will cause additional distortion of the normal innervation pattern.

Thus, in summary, the nature of nerve outgrowth, limitations in nerve splicing methods, and unavoidable obstacles inherent in accidental nerve injuries, all tend to preclude any perfect restoration of peripheral nerve linkages. Distortion of the normal innervation pattern is always in some degree inevitable and, particularly in clinical practice, a completely haphazard reshuffling of relations between the central nervous system and the end-organs supplied by the divided

nerve is often unavoidable. The disarrangement will be the greater, the larger the nerve trunk involved and the more heterogeneous the function of its constituent fibers and end-organs.

Even more abnormal are the terminal connections resulting from nerve crossing, i.e., the union of the central stump of one nerve to the distal stump of another. Although employed more liberally in experimental studies, nerve crossing is not uncommon in clinical surgery in cases where it is impossible to use the central stump of the damaged nerve (Stookey, 1922; Pollock and Davis, 1933). Nerve crossing deliberately forces all the regenerating fibers to terminate in foreign end-organs, which may be far removed both in anatomical location and in mode of function from the nerve's original endings. In addition to this wholesale switching of nerve fibers from one region to another, nerve crossing involves also, like reunion of the ends of the same nerve, a disorderly redistribution of individual fibers within the nerve itself. This internal reassortment of fibers of different function within the reinnervated area is worse after nerve crossing than after direct reunion, because there usually is no correspondence whatever between the intraneural fiber pattern of the distal stump and that of the central stump. The outmoded method of lateral implantation and the crushing and severance methods (Kilvington, 1905; Dogliotti, 1935; Aird and Naffziger, 1939; Billig and van Harreveld, 1943) for utilizing the excess branches of regenerating fibers to supply an extra load of end-organs also deliberately foster confusion in the innervation pattern.

To what extent, if any, can central nervous reorganization compensate for, and correct, the sensory and motor dysfunction which otherwise must inevitably follow such rearrangements of peripheral nerve connections? Compared to the surgical problems, histological aspects, and immediate physiological results of the treatment of peripheral nerve lesions, the qualitative problems of functional recovery raised by the shunting and crossing of nerve fibers have been largely neglected. On the motor side, the problem is that of readjusting the timing and rate of discharge of misdirected motoneurons. On the sensory side, the problem is one of altering in accordance with their new end-organs the central sensation and response associations of the misdirected afferent fibers. This problem of recovery of function in natural systemic activities is to be distinguished sharply from the mere re-establishment of transmission of excitation from nerve to end-organ or vice versa, recovery of muscle volume and contractility, quantitative recovery of general sensibility, etc. Thorburn (1920a) has justly contrasted mere physiological success with what he called the "economic" success of nerve regeneration. More recently Sanders and Young (1942) and others have stressed our present lack of knowledge regarding the qualitative aspects of recovery and the importance of giving them more heed in the future. The general lack of reliable information regarding the functional results of misdirection of nerve fibers in regeneration is becoming recognized as a definite handicap to more rapid progress in the treatment of peripheral nerve lesions.

The present survey does not devote any primary consideration to those functional recoveries effected through compensatory adjustments in the remaining intact or sound parts of the organism. Just as it is possible to compensate to some extent in various performances for the complete loss of a limb, so it is possible to compensate for paralysis or dysfunction of a limb or its parts. The underlying central nervous reorganization involved in such compensatory readjustments in the sound parts of the system is of a more common and simple type than that generally required to readapt directly the function of the disarranged nerves and end-organs. Also such compensatory adjustments at their best cannot be expected to approximate the optimum recovery which would be achieved by complete restoration of adaptive function to the affected nerves and end-organs themselves. Whether or not the abnormally innervated or transposed end-organs can themselves come to function again in a proper adaptive manner as a result of central nervous reorganization poses at present the primary and more fundamental problem, from both the practical and theoretical viewpoints. It is this aspect of recovery with which the present review is specifically concerned.

Previously, this problem has received attention largely for its bearing on theoretical questions pertaining to such subjects as central nervous plasticity and adaptability, anatomical localization of central readjustments, equipotentiality vs. specificity of central reflex associations, peripheral vs. central control of coordination, innate vs. acquired nature of central and peripheral relations, selectivity of nerve regeneration and termination, the manner of ontogenetic development

of connections between center and periphery, and related topics. The problem also has immediate practical significance in the treatment and handling of peripheral nerve injuries and paralyses of different kinds; for example, in the choice of surgical methods and procedure, in determining the possibilities and limitations of functional repair by muscle and nerve substitutions, in constructing reeducation and rehabilitation programs, in evaluating compensations for disability, and wherever prognosis of the quality of functional recovery is important.

Previous views and conclusions

In the past an overwhelming majority have concluded that complete recovery of normal function after nerve crossing and regeneration as well as after muscle transposition is definitely possible and regularly occurs not only in man but in the lower mammals as well. The most extensive review of the subject is that by Bethe and Fischer in 1931. After summarizing all the available studies, they emphasize with italics the conclusion that there can be no doubt from the existing evidence that the nervous system can adjust fully and completely to changes in the anatomical connections brought about by nerve interchanges. They conclude that not only correct motor coordination, but also correct sensory localization is usually recovered after peripheral nerve exchanges in other animals as well as in man. From the rapidity of the restoration of correct function reported frequently to follow nerve crossing and muscle transplantation, they deduce further that central nervous integrative processes are conditioned much more by peripheral connections than by connections in the central organ itself.

Anokhin and his collaborators have devoted many years to the study of the functional results of nerve regeneration and interchange of peripheral nerve connections. In their monograph (1935a) they maintain that they have demonstrated that organs which have been supplied with an unusual innervation do acquire in the end their regular function. They believe that the normal distribution of nervous impulses in the centers is not at all rigid and unchangeable but very mobile, and even at the spinal level is capable of complete adaptive reintegration after alteration of the phylogenetic connections between central nervous system and periphery. Later (1940), Anokhin again asserts, on the basis of the results of nerve crossing, that the specificity of the spinal motor centers is not fixed and constant but depends upon the connections with peripheral organs.

Foerster (1930), after long clinical experience with recoveries from nerve injuries and after clearly schematizing in some detail the extensive shifts in central nervous associations from the cortex to the spinal centers that are necessary for recovery after nerve crossing as well as after straight reunion, states that recovery nevertheless occurs and that the new relationships that have to be formed in the centers come eventually to function as well as did the pre-existing normal relations.

Stopford (1930), as did Osborne earlier (1909), assumed that reeducation is possible after misdirection of sensory nerve fibers and explained the delay in reappearance of so-called "epicritic" sensation after nerve regeneration on the basis of a reeducation period in which the higher brain centers are adjusted to the new afferent terminations.

Lee, in his 1929 review of nerve regeneration, described the aberrant outgrowth of nerve fibers and after pointing out that, due to shunting in the scar region, every end-to-end nerve suture is really an example of multiple cross unions, he concluded with the assumption that reeducation within the central nervous system probably smooths out the dysfunction produced by these wholesale anatomical aberrations.

Perthes (1922), in his review of functional recovery after nerve injuries of the first World War, introduced his discussion with the statement that experimental nerve reunion in animals leads with certainty to complete recovery. He also emphasized the extreme suddenness with which readjustment may occur in human patients after abnormal reinnervation as well as after muscle transposition (Perthes, 1918), and referred to the rapid learning of the brain to adjust to the new anatomical relations.

Kennedy published, with thorough reviews of the earlier literature, a number of extensive experimental and clinical observations over a period of 20 years beginning in 1897, on the problem of central readjustment after nerve regeneration. He remained strongly convinced on the basis of the experimental and clinical data that normal function, both motor and sensory, is restored following the cross union of peripheral nerves as well as after their straight reunion. In one of his last reports (1919) he listed a number of clinical cases in which, after complete severance of nerve

trunks of the arm, sensation and motion were restored so as to give restoration of normal function. He said further that recovery after complete division of nerves was not different from that after simple compression, which latter causes interruption of nerve fibers but does not lead to disarrangement of the innervation pattern. Kennedy's views and experiments have frequently been cited in the clinical literature as evidence that reeducation may effect complete recovery following abnormal reinnervation.

Spitzy (1908), after considerable study of functional recovery after nerve crossing, both experimental and clinical, and with some appreciation of the complications involved in compensating for the anatomical confusion inevitably wrought by nerve crossing, was moved, in describing successful recovery in some of his patients, to refer enthusiastically to the "wunderbare Selbstregulierung" and "kolossalen Anpassungsfähigkeit unseres Nervensystems."

Of the even more numerous reports of good functional recovery after muscle transposition, those by Marina (1912, 1915) are outstanding. He concluded on the basis of the results of muscle transposition in man, and particularly on the basis of his experiments on reciprocal antagonistic transposition of ocular muscles in the monkey, that the functions of central association pathways and intranuclear connections of the brain and cord are not rigidly specialized, as traditionally assumed, but are entirely plastic and dynamically regulated. He made a strong plea for a complete revision of our central nervous physiology from the ground up, to bring it into accord with the extreme dynamic adaptability which he had observed.

The above reports and generalizations are a few among many which would endow the central nervous system with special and very remarkable capacities for maintaining functional proficiency in the presence of the extreme anatomical chaos that follows nerve crossing and regeneration and the less drastic disarrangements of muscle transposition. In view of the existing literature, Goldstein (1939) was not unjustified in his discussion of the problem in stating that such central nervous reorganization occurs very readily, often spontaneously without any training. He believed that the evidence fully supported his contention that it is immaterial what particular nerve fiber connections exist between center and periphery; that so long as any connections whatsoever are present, correct function follows. The practical as well as theoretical import of such conclusions as the above is obvious. Even such a practice as dissecting out full length the healthy ulnar nerve of the arm and crossing it to the leg nerves to restore function to the lower limbs of patients paralyzed by cord transection, as undertaken by Krukenberg (1918), might be considered justified by the earlier literature.

Although these views have long prevailed, their acceptance has not been entirely unanimous. Occasional dissenting opinions and objections are to be found scattered through the literature. Recently, on the basis of clinical evidence, Ford and Woodhall (1938) have questioned quite strongly the prevalent assumption that reeducation may compensate for misregeneration. Moreover, within recent years a number of controlled mammalian experiments designed to further analyze central nervous adaptation to disarrangement of the normal relations between center and periphery (Sperry, 1940-1943) have yielded results in direct contradiction to the optimistic views cited above. Instead of complete rapid reorganization to suit new peripheral relations, nerve-muscle rearrangements were found to result in intractable perverse discoordinations and maladaptive reversals of response which in most cases persisted indefinitely with no sign of correction. Results equally clear obtained in amphibians (Weiss, 1941c; Sperry, 1943a-1944) have also proven irreconcilable with the contentions of Bethe, Anokhin, Goldstein, Marina and their followers regarding the basic plasticity of the vertebrate nervous system and the absence of any fixed functional specificity in the linkages between center and periphery and between and within nuclei of the cord and brain.

The recent contradictions, in conjunction with other scattered reports of persistent malfunction particularly following nerve redistribution, cast doubt upon the long prevalent generalizations and indicate that the problem is by no means satisfactorily settled. Critical reexamination of the evidence and a thorough reconsideration of the entire question is particularly urgent at the present time because of the large number of cases of peripheral nerve lesions coming in for treatment, the consequent impetus to research on related phases of nerve regeneration, and increased opportunities for further study of the late, qualitative aspects of recovery. Because of the controversial status of the problem and because the material has not previously been collected and adequately

summarized, a fairly extensive survey is offered. Rather than to attempt to draw any definite conclusions, the main purpose of the present paper is to gather and review critically the bulk of the widely scattered reports bearing on the subject.

EFFECTS OF INTERCHANGING MOTOR NERVES

Although there is a tremendous literature on the results of surgical reunion and crossing of nerves, the major portion of it is of no consequence to the problem of central nervous readaptation, because the qualitative aspects of recovery were not considered and no pertinent data were recorded. It will become clear that most of the evidence, both clinical and experimental, has been drawn in the past from cases of nerve crossing rather than from cases of straight reunion, even though, as mentioned above, every case of nerve reunion is actually a case of multiple cross-unions of different fiber types within the nerve trunk. The reasons are several. First, after crossing nerves, all fibers are directed to foreign end-organs. After reunion of the ends of the same nerve, on the other hand, there is always a probability that a certain percentage of fibers have returned to their original end-organs, leaving it doubtful how much of the recovery of normal function is due to correct regeneration and how much to central adjustment. Second, determination of the quality of motor or sensory recovery is usually at best difficult. Crossing nerves of widely different function whose terminations are well separated leads to a functional result that is much more easily detected and evaluated than are the more subtle defects that follow straight reunion of a single nerve. Third, after straight reunion, the multiple branching of single fibers to several end-organs, often of diverse function, may permanently prevent any dissociation in the function of these end-organs. It may thus be doubtful after straight reunion whether persisting association of movements or sensation is due to lack of central plasticity or to excess branching of single fibers. After successful end-to-end nerve crossing, by contrast, single fibers are not able to send branches to both the original group of end-organs plus the new group, so that any persisting functional association between the new and old regions supplied by the crossed nerve cannot be blamed on fiber bifurcation. For these reasons most of the experimental, and also the most decisive clinical evidence, has been derived from cases of nerve crossing rather than of straight reunion.

Although regeneration of most peripheral nerves presents a problem both of sensory and motor recovery, and although the readjustment process itself may necessarily involve inseparable sensory-motor mechanisms, the evidence related to misdirection of motor fibers on the one hand, and of sensory fibers on the other, is here separated for convenience of discussion.

Flourens' Classic Experiment

It is generally agreed that the first attempt to study the results of crossing nerves was made by Flourens (1828). In a cock, he cut and crossed the two main nerves leading from the brachial plexus to the ventral and dorsal aspects of the wing, respectively, and reported that after a few months the bird recovered use of the wing so that it could fly as well as before. This first, classical experiment has been repeatedly cited over since as an example of the recovery of coordinated action after switching the innervation of antagonistic muscles.

In considering the recovery of flight in this case, it must be remembered that the main muscles of flight acting on the humerus of the wing were not affected by the operation. It is probable that the muscles acting on the elbow joint of the wing also retained their complete normal innervation, for they are supplied by small nerve twigs high under the shoulder, and furthermore, Flourens himself indicates that his operation affected only the extremity of the wing. The tensor muscles of the patigial membrane of the wing were also probably not cut in the operation, according to Cunningham (1898), who suggests that these muscles, rather than the test muscles, may have been primarily responsible for the recovery. Flourens failed to give a detailed description, but it thus appears very probable that only the small muscles acting on the carpal joint of the wing were affected by the operation.

The action of the carpal joint of the wing in the flight of the domestic cock is not a very delicate indicator of the differential reciprocal function of antagonistic muscles. With normal action of the proximal joints, a simple sustained rigidity of the carpal joint would be sufficient to give to most observers the impression of normal flight. Moreover, the ligaments and tendons of the cock's wing are such that extension of the elbow joint, as

in flight, secondarily produces a full extension of the terminal wing segment accompanied by a firm spreading of the large distal wing feathers. Flexion at the elbow releases this passive extension of the carpal joint and permits carpal flexion. With this secondary distal action of the elbow muscles, plus the direct action of the elbow muscles and also of the main flight muscles of the shoulder and finally of the patigial tensors all remaining intact, it is not surprising that good use of the wing was recovered.

Stefani (1886) mentions in regard to Flourens' experiment that the normal flight of the pigeon is not noticeably affected by cutting either one of the two main nerves of the wing. Drooping of the distal wing segment is the most obvious defect that would be anticipated after Flourens' operation. It is quite possible that nerve regeneration bringing return of muscle tonus and contractility and perhaps also contracture would remedy this defect and at the same time restore sufficient rigidity to the extended joint to greatly improve its use in flight without any reorganization of central nervous coordination. Schiff (1885) has pointed out that Flourens' description was too short to give any indication as to whether the action of the reinnervated muscles was actually coordinated or not. Cunningham (1898) questioned the purity of the crossed regeneration, suggesting that stray fibers may have reentered their original distal nerve paths. Flourens' physiological checks, made without anesthetic or surgical elimination of reflex responses, were hardly adequate to determine this point. In view of these various objections it is obvious that no reliable conclusion regarding central readjustment can be drawn from this single historic experiment of Flourens.

About a half century after Flourens' initial experiment, attempts to repeat nerve cross operations were successful and appeared in the main to confirm Flourens' observations. This apparent experimental success with nerve crossing was soon followed by clinical application, and ever since then numerous scattered reports, both clinical and experimental, have been accumulating in the literature. Although the clinical accounts now far exceed the experimental reports in numbers, it is the latter which in most instances have yielded the more thorough and more widely accepted evidence on the problem of central nervous reorganization.

Results on Hind Limb

Experimental

The sciatic nerve of the hind leg and particularly its two terminal branches, the tibial and common peroneal nerves (internal and external popliteal nerves) have been used frequently in experimental nerve crossing. These two divisions of the sciatic are relatively large nerves, readily accessible, and lie side by side for a considerable distance, making their cross-union easy from a technical standpoint. And, particularly important with respect to the problem of central reorganization, they innervate antagonistic muscle groups. The main dorsiflexor muscles of the ankle joint are supplied by the common peroneal, and the main plantar extensors are supplied by the tibial. Accordingly, a number of studies of partial or complete crossing of these nerves have been undertaken with a view to demonstrating the functional effect of switching the innervation of antagonist muscle groups. At first glance, complete crossing of these nerves might be expected to produce a reversal of foot movement, i.e., dorsi-flexion when plantar extension should normally occur, and vice versa. Instead of such a reversal of foot movement, however, nearly all investigators who have performed complete or partial exchange of the normal nerve connections in this region have until the last few years reported a rapid and complete recovery of normal leg movements.

Complete reciprocal crosses between the common peroneal and tibial nerves were undertaken by Rawa (1885) in cats and rabbits, and by Spitzy (1905) and Osborne and Kilvington (1910b) in dogs. Instead of crossing separately the peroneal and tibial nerves, Kennedy (1899) rotated the distal end of the divided sciatic in the dog 180 degrees and sutured the stumps so that the proximal tibial division was apposed to the distal peroneal division, and vice versa. For comparison, he also reunited the divided sciatic in correct orientation. Separate crosses between tibial and common peroneal in one direction only, i.e., proximal tibial to distal peroneal or the reverse, were made by Rawa (1885) both unilaterally and bilaterally in dogs, cats, rabbits, and pigs.

The proximal stump of either tibial or peroneal nerve was sutured to the distal ends of both peroneal and tibial nerves in the dog by Kilvington (1905) and later by Aird and Naffziger (1939). In these experiments the nerve fibers normally supply-

ing one set of muscles were made to reinnervate not only their original muscles but, by multiple axon branching, their antagonist muscle group as well. Dogliotti (1935) also sutured, in the dog, a small undesignated portion of the proximal stump of the divided sciatic to the entire distal stump. On the basis of apparently successful results in the latter three studies it has been suggested that, in cases of poliomyelitis or of other neurogenic paralyses, a few remaining healthy fibers may be made to reinnervate profitably an extra load of muscles (Billig and van Harreveld, 1943). A somewhat similar effect was secured by Spitzy (1905) in dogs by implanting the end of the distal portion of the divided peroneal into the intact tibial or vice versa, so that severed fibers in one nerve were guided in regeneration to the end-organs of the other nerve with the possibility of additional branches reaching the original muscles also.

Contralateral crosses between the left and right sciatic nerves were performed by Bethe (1905; Bethe and Fischer, 1931) in the dog, with the result that the right leg, at least, became successfully reinnervated by the left sciatic nerve. Contralateral crosses between the sciatic nerves of the dog were also madé by Maragliano (1911, 1912). He split the main sciatic trunk in the right leg and crossed one division to the entire sciatic trunk of the left, so that after regeneration the sciatic musculature of both legs was innervated from the right sciatic spinal centers.

Despite the abnormal switching of connections between the central nervous system and the hind limb muscles in the above studies, it was reported in all cases that the animals recovered normal leg movement. Most of the above investigators gave special attention to the problem of the dissociation and reassociation of motor patterns and insisted that normal muscular coordination was perfectly reestablished. Thus it has been asserted that after complete reciprocal crossing of the peroneal and tibial nerves, and after various types of partial crossing and intermixing of the nerve connections of plantar extensor and dorsi-flexor muscles, and even after contralateral nerve crosses between left and right leg, restitution of normal motor function is possible and does occur in the majority of cases. The more outstanding of the above reports have long been widely accepted as conclusive demonstration of the extreme dynamic plasticity and reintegrative capacities of the central nervous system. The recovery of normal function after the switching of these particular nerves is all the more striking since it involves modification of those presumably stable functional associations of the spinal centers underlying the classical hind limb reflexes upon which the familiar laws of reciprocal innervation of antagonistic muscles have been largely based (Sherrington, 1906). It has been taken for granted from these results obtained mainly on the dog that, provided equally successful regeneration of severed nerves could be attained in man, "relearning" might be expected to occur even more readily.

However, certain objections and criticisms to the entire series of above investigations prohibit acceptance at present of any conclusions drawn therefrom regarding central nervous plasticity and reorganization. In none of the above studies was there any detailed analysis of the action of individual muscles before and after operation. The criteria of functional recovery included merely the animal's general use of the limb in walking, running, jumping, and other activities, with at best only a comparison of the use of the operated limb with that of the contralateral unoperated limb. Also there were rarely control experiments to eliminate alternative interpretations. In fact, detailed consideration of exactly what defect the operations would be expected to produce and how much recovery would be anticipated simply from reinnervation without any central readjustment makes it seem very questionable that any central reorganization was involved in the results.

In evaluating this group of experiments, it must, first, be kept in mind that the lower sciatic and peroneal and tibial nerves are distributed to muscles below the knee only, so that it is merely movements of the foot and digits which were affected by the nerve-cross operations. Function of the hip and knee joints was not appreciably impaired. Furthermore, in the animal species used, the dog, cat, rabbit, and pig, movements of the digits in locomotion and other general activity are so negligible that abnormalities of action could easily escape notice. Hence it is primarily to movement of the ankle joint alone that the reports of functional recovery may be said to apply. The ankle joint, by virtue of its structure in these animals, is largely restricted in its range of movement to flexion and extension. There is little need for active dorsi-flexion of the foot in these digitigrade animals in most general activities; the ankle is simply kept in extension and used like a stilt for support. Thus the main requisite for restoring to the dog the use of the hind leg after such operations

is a sustained extension of the ankle with sufficient stability to support the body weight.

Such plantar extension with, at the same time, enough resiliency for dorsi-flexion to occur to some extent in the correct flexor phase of coordination is exactly what might be predicted in the absence of central readjustment to follow the complete crossing of the peroneal and tibial nerves, for the following reasons: The peroneal and tibial nerves innervate in addition to the primary flexor and extensor muscles of the foot many other muscles of the shank, including those of inversion and eversion, and those for spreading and flexing the toes, as well as the array of small intrinsic foot muscles. It may be safely presumed that all these additional muscles do not normally work in perfect synchrony with the two primary groups in strict association with one or the other according to their peroneal or tibial innervation. Nor does it necessarily follow, because the primary dorsi-flexor and plantar extensor muscles are larger in mass than the other shank muscles, that the action of their fibers should predominate after crossing. There is evidence that smaller muscles have many more motor fibers per mass unit than do the larger muscles (Clark, 1931), so that the small muscles figure much more prominently in the nerve fiber population than they do as muscles. Moreover, axons which originally supplied very small motor units in small muscles may regenerate into large muscles and come to command motor units of much greater size and strength than those originally supplied. An additional factor contributing to functional confusion is the tendency of individual axons to send multiple branches to different muscles. In view of the foregoing, the crossing of these compound nerve trunks could be predicted schematically to result only in a confused contraction en masse of all reinnervated muscles supplied by both nerves in any leg movement.

Due to the superior strength and mechanical relations of the antigravity plantar extensor muscles, such massive indiscriminate excitation of the shank musculature would lead to plantor extension of the foot, a tense, stiff extension because of the opposed contraction of all muscles about the shank. This extension would probably be further enhanced in time by a permanent contracture of the plantar extensor muscles. The ankle joint seems to be particularly prone to the development of such contractures (Kennedy, 1919; Pollock and Davis, 1933; Sperry, 1941; and others). Massive contraction of the extensors, aided by a contractural shortening of these muscles in adjustment to the natural walking position of full extension, would furnish ankle support possibly even stronger than normal. Though lacking perhaps in adaptable gradations of tension, it would still be somewhat resilient.

Maintenance of plantar extension would be further reinforced in the supporting phase of leg movement in locomotion by extension of the knee. The major plantar extensor muscles inserting on the Achilles' tendon of the heel have their origin on the femur above the knee joint so that extension of the knee simultaneously forces a strong passive extension of the ankle. Conversely, flexion of the knee releases this force and permits the ankle to flex dorsally. Flexion and extension of the knee would thus result in simultaneous passive flexion and extension of the ankle. Since the knee and ankle normally work in unison in these animals, the effect would closely simulate normal function. Such passive movement of the ankle by the knee would show marked improvement after reinnervation had restored tonus and contractility to the paralyzed shank muscles. This gradual recovery of stability of the ankle joint, and even a tendency for passive ankle flexion and active extension to occur in the correct phase of limb movement, would permit a use of the hind limb which to gross observation might well be indistinguishable from normal. Certainly, compared to the complete paralysis immediately following nerve section, or to reversed or spasmodic movements of the joint such as might at first thought be expected to follow nerve crossing, it is understandable that recovery of the use of the limb to this extent might well suggest to many observers that central readjustment had occurred.

Thus the reports on the results of interchanging experimentally the peroneal and tibial nerve connections are open to the general criticism that the use of the leg that appeared to be executed by normal muscle coordination might well have been effected passively by movement of the knee aided by contracture of the plantar extensor muscles, without involving any adaptive readjustment whatever in the central motor coordination patterns. Further points may be cited in support of this interpretation. Schiff (1885) very early objected to Rawa's experiments on the basis that the foot movements were passive, produced by the action of the knee joint. Kennedy (1901, 1914a, 1914b), in later reports following his own earlier work on the sciatic nerve, stated that the reported

recoveries in the hind limb leave doubt as to whether they were real or apparent, because after complete section of the sciatic nerve high in the thigh at the level of the trochanter the dog is still able to use the hind leg in walking. He said that the ligaments of the hind leg in the dog are sufficient to support the animals without muscular contraction. He mentioned further that the most noticeable defect after sciatic nerve section in the dog is a tendency for the dog to walk on the dorsum of the paw, but that this is not always present, and if it is, it shows improvement anyway without actual reinnervation of muscles, giving a false impression of recovery. Also Swan (quoted by Sherren, 1906a), after studying the results of experimental section of the sciatic nerve in dogs and rabbits, wrote, "I was at first astonished at how much an animal could move its limb a short time after operation and concluded that misconceptions have arisen from considering the general motion of the limb as indications of the restoration of the nerve."

The unreliability of the above reports of recovery of muscle coordination is further illustrated by the fact that in some cases it was reported (Rawa, 1885; Kennedy, 1897) that good recovery of function occurred within so short a period after operation that the nerves could not possibly have had time to regenerate and reestablish functional connections. Such an obvious error in interpretation is explained by the fact that many of the experiments were carried out before it had become generally recognized that severed nerves, instead of healing together directly, have to regenerate new fibers all the way from the central stump to the periphery (Boeke, 1935).

The applicability of the above criticisms to the experiment of Osborne and Kilvington (1910b), in particular, might be questioned because they studied the hind limb reflexes of their animals after cord transection, mentioned that great care had to be taken to avoid confusion due to mechanical effects of movement of associated joints, and stated specifically that some of the reflex acts they obtained could not possibly be regarded as mechanical and passive. They crossed the peroneal and tibial nerves unilaterally in four young dogs. At first after regeneration there were signs of reversed movements and much incoordination, but slowly the animals learned, in a manner suggesting to the authors "conscious effort and analysis," to effect coordinated movement, until at the end of a year the movements of the operated limb were reported to be as rapid and coordinated as those of the sound limb in walking, running, galloping, and jumping. The spinal cord was transected about a year after the nerves had been crossed, and it was then found that the reflexes of both hind limbs were identical indicating that the corrected coordination of the operated leg had persisted in the spinal state. It was concluded from this that the neural basis of acquired habit may extend to the lower levels of the spinal cord. The experiments were undertaken originally to test whether new acquired coordination patterns can predominate over innate patterns, and it was decided from the results that this is possible if sufficient time (about a year in these cases) is allowed for training.

This experiment is generally regarded as the most convincing of the above series. The observation that coordination was not only recovered but that it persisted after cord transection, as if reorganization had occurred in the lower spinal centers, is extremely significant. The interpretations and conclusions of the authors, however, are hardly acceptable at present without further proof. Of their four animals, one was discarded because the operation was faulty, two others did not survive the spinal transection in good condition, so that apparently only one animal yielded a fully satisfactory record of spinal reflexes. In this case, later anatomical check revealed that some fibers from both nerves had grown back into their own distal stumps, although the action of these fibers was said to be so trifling as to be considered negligible. Despite their statement regarding great care to avoid misinterpretations due to passive mechanical effects from neighboring joints, no specific reason is given why the foot movements they observed might not have been mechanical, nor is mention made of any definite precautionary measures to eliminate such sources of error.

In addition to flexion and extension of the foot, in one case a type of toe-spreading reflex was elicited which was said to be identical in both hind legs. Gutmann (1942) also has demonstrated recovery of toe-spreading in the rabbit after crossing the tibial nerve to the distal stump end of the peroneal. It appeared in the rabbits, however, only when they were held upside down, was very poor at best, and could be elicited only with difficulty. That the recovery of toe-spreading in the upside-down position was due to practice or reeducation is doubtful, and Gutmann furthermore explains that the assumption of central reorganiza-

tion is unnecessary, since there are motor fibers present in both tibial and peroneal nerves mediating this reflex. One wonders further if muscles innervated by the tibial nerve other than the single muscle involved in toe-spreading are not also active normally in the particular inverted conditions in which the reflex appeared. Fibers of such muscles rerouted into the peroneal group would also produce toe-spreading without need for central adjustment. That a similar explanation is applicable to the results in Osborne and Kilvington's dogs is also possible, but this cannot be said with certainty because the reflex they mention is unfamiliar and was not described in detail.

In any case, the conclusions of Osborne and Kilvington have not been borne out in recent reinvestigations. Watrous and Olmsted (1941) studied the late functional results of crossing the peroneal and tibial nerves in cats and dogs by recording after decerebration the reflex contractions of isolated flexor and extensor muscles. Although the results tended to be somewhat ambiguous and the report has since been cited both for and against reeducation, it would seem a safe deduction that at least the recovered responses were definitely abnormal and incoordinated. Even in control cases in which the nerves had been reunited to their original stumps, the reflex reactions were incoordinated. Obrador (1942a) also crossed reciprocally the peroneal and tibial nerves in cats and dogs. Observation of the animals' stance and gait after regeneration revealed signs of defective posture and incoordination, but the withdrawal reflex and in some cases the support reaction of Magnus were not abnormal to gross inspection. In kymograph recordings of the contraction of individual muscles after decerebration or under anesthesia, contraction of the plantar extensor muscles predominated whether ipsilateral or contralateral afferent nerves were stimulated. The presence of extensive axon reflexes involving muscles of both nerve trunks reveals that Obrador did not achieve his purpose of clearly crossing the two nerves. Even so, he concluded that no visible signs of a marked reorganization and functional readaptation in the spinal levels appear after peripheral nerve crosses. The tests of Watrous and Olmsted and of Obrador after decerebration give no assurance that motor coordination had not been present previous to decerebration. According to what evidence we have at present (Lashley, 1921) regarding the learning process and automatization of habits, decerebration or cord transection would be expected to abolish any reeducational adjustments that might have existed. The results of these more recent tests of decerebrate reactions nevertheless definitely discredit Osborne and Kilvington's previous findings of complete reorganization in the lower spinal levels.

The functional effect of crossing the peroneal and tibial nerves has been examined in the rat by Sperry (1941). Twenty animals were operated on, at ages ranging from 15 days to 80 days. The strength of foot movements upon electrical stimulation of the nerves, as well as the size of the shank muscles 2½ months after operation, indicated that excellent crossed reinnervation was achieved. In none of the twenty cases, however, was there a restoration of normal coordination. All animals exhibited a pronounced stiffness of ankle movement with plantar extension predominating. The picture suggested massive undissociated contraction of all reinnervated muscles. Active dorsi-flexion of the foot, although it could be produced by electrical stimulation of the crossed tibial nerve, never occurred naturally either in reflex or voluntary movement. Gutmann (1942) has since reported a similar result in the rabbit. In the course of time the tense incoordination in the rats, instead of approaching normal reciprocal movement, gradually became worse. Especially in the younger, more rapidly growing rats the persistent extension of the ankle resulted ultimately in an anatomical shortening of the plantar extensor muscles until the rats walked about on their digits with the ankle fully extended. These results are particularly interesting because the nature of the recovery, obviously maladaptive in the rat, would be quite beneficial and would closely simulate the normal limb action of a digitigrade animal like the dog, on which most of the older experiments were carried out. No sign of reeducative correction of the function of the crossed peroneal and tibial nerves was evident in the rat.

It has been shown further that with more carefully designed operative measures, the crossing of nerves to the flexor and extensor muscles of the shank in the rat results in a clear-cut reversal of ankle movement (Sperry, 1941). In 10 animals, 8 operated on unilaterally and 2 bilaterally, individual muscular nerve branches to flexor and extensor muscles were dissected free and crossed to specific antagonist muscles, the anterior tibial muscle and the gastrocnemius muscle. In addition, all remaining muscles acting on the ankle were excised to prevent their action from masking

that of the test muscles. This crossing of individual muscular nerve branches, instead of the whole peroneal and tibial trunks with their heterogeneous combinations of fibers of varying function, accompanied by excision of all non-test muscles acting on the joint, tended to produce a more precise result and at the same time eliminated several major sources of trick movement.

Under these more critical conditions the functional effect after nerve regeneration was a clear-cut, positive reversal of foot movement in all activities, voluntary as well as reflex. Dorsi-flexion occurred when extension would have been serviceable, and plantar extension when flexion should normally have occurred. This diagrammatic reversal of foot movement never reverted to normal. It persisted without any signs of correction despite training conditions and surgical measures designed to force out reeducational adjustment. The majority of cases were kept until the approach of senescence. They failed not only to make a positive correction of the maladaptive responses, but even to inhibit the reversed reactions, which were decidedly more detrimental than would have been no movement at all.

Failure to correct such obviously maladaptive reversed movements is to be distinguished from failure to correct less serious motor incoordinations in situations in which, as after crossing the peroneal and tibial nerves in animals like the dog and cat, abnormality of movement is hardly manifest and detracts only slightly from the animal's use of the affected limb. The incentive for education afforded by markedly reversed movements is much stronger, and hence lack of readjustment under these conditions is more striking proof of the lack of readaptation capacity. It must also be emphasized that the anatomical disarrangements and the central reorganizations required to correct them are much less extensive in character after nerve crosses involving individual muscular branches, than after the unselective crossing of large heterogeneous nerve trunks. The central readjustment required after crossing individual muscular branches may be compared with that required after muscle transposition (see below), in contrast to the complete dissociation and reintegration from the neuron level up needed after haphazard regeneration and crossing of compound nerves. Finally, the rats' failure to improve their condition by simply inhibiting all foot movements is to be distinguished from the failure to achieve the more complex task of effecting positive reestablishment of correct movements. That the rats did not learn even to suppress the reversed responses either in simple deliberate movements or in reflex reactions under the above conditions is particularly convincing evidence of the limitations of central adaptation capacity.

Besides the peroneal and tibial nerves of the hind limb, the obturator and femoral nerves have also been subjected to partial and complete crossing. These nerves innervate antagonistic muscles of the thigh, the obturator nerve supplying muscles for adduction and flexion and the femoral nerve muscles for abduction and extension. Spitzy (1905) produced partial crosses between these nerves in the dog by implanting the distal end of the divided crural nerve into the obturator. He reported good recovery of locomotion. A more thorough study of the effect of crossing these nerves has been made more recently by Anokhin and Iwanow (1936b; Anokhin, 1935a). They made complete reciprocal crosses in the dog, both bilaterally and unilaterally. During the first two months after operation the dogs were unable to support themselves on the operated legs. Beginning about the third month, the use of the hind limbs gradually improved, until at the end of a year locomotion was indistinguishable from that of normal animals. Even in the absence of the corresponding area of the motor cortex a positive recovery was finally achieved, although the reintegration was said to proceed more slowly. The authors concluded that the flexor and extensor motor horn cells had changed function as a result of local reintegration processes within the spinal cord, and that later, following the order of ontogenetic development, these new spinal patterns were brought into relation with cortical processes.

That any central reintegration either in the spinal cord or at higher levels had actually occurred in the animals of Anokhin and Iwanow may justly be questioned on the following grounds: According to the authors' diagrams of the anatomical conditions before and after operation and after regeneration, the nerve crosses were not distinct. A sizable bundle of fibers of the sciatic nerve was crossed along with the obturator nerve into the femoral muscles. Also the precautions taken to prevent regeneration of fibers back into their own distal stumps were not successful. Moreover, the nerves crossed do not innervate the entire thigh musculature acting upon the knee joint. Strong flexion of the knee would still be possible, for the nerve supply of the main flexor muscles remained

intact. This would explain the good flexion reflex to electric shock which was reported to be present after recovery. The most serious detectable effect of the operation would be inability to use the knee for support, due to paralysis of the quadriceps extensors. The recovery of tonus and possibly the establishment of a persistent contracture in these muscles after reinnervation by the obturator nerve, apart from any central reorganization, would aid greatly in restoring strength and steadiness to the joint, and thereby its use in locomotion. It has not been shown and cannot be assumed that the muscle groups supplied by the obturator and femoral nerves contract in perfect antagonism in the locomotion of the dog. Considerable co-contraction may be present in the muscle groups of these two nerves, with the result that the knee extensors after reinnervation would naturally be activated to some extent in the appropriate phase of limb movement. This possibility of the quadriceps having been reinnervated by motor fibers whose normal central timing was not inappropriate for these muscles was further favored by the inclusion with the obturator nerve of fibers belonging to the sciatic trunk. Furthermore, the action of sound muscles acting on the thigh and lower leg when these are fixed at one end by the ground and at the other by the trunk could exert considerable extensor force at the knee joint. Watrous and Olmsted (1941) have suggested that possibly normal use of the limb in these experiments was brought about by muscular compensation In this connection, J. E. Stewart (1925), after transplanting the nerve supply of thigh muscles in the dog, found that dogs in which the sartorius muscle had been cut and the rectus femoris paralyzed, got about perfectly normally after a few weeks, showing no limp whatever in walking. He went on to warn that, as a consequence, experiments the results of which are determined by the ability of the animal to use the extremity would seem to be unreliable. Anokhin and Iwanow did not check these alternative possibilities of interpreting the recovery they observed.

It is thus quite conceivable that recovery of the use of the limb in locomotion in these cases may have been due to the compensatory function of sound muscles plus massive undifferentiated contraction or contracture of the affected muscles, imparting a generalized background stability to the affected joint. With such stability, absence of normal muscle coordination would be difficult to detect in locomotion and general activity where the lower leg needs to serve only for support. Incoordination, if present, however, might show up in specialized movements, and in this regard it is truly "interesting," as the authors state in describing a typical case, "that two years after the establishment of new relations in the spinal cord, the animal moves and runs like any normal animal but is nevertheless incapable of performing the finer movements which require the work of specific muscle groups." They conclude that movements of the extremity in the total locomotor act are therefore different from isolated movements performed when the animal is at rest. This is very probably true, but it is doubtful whether the rapid automatic complicated coordination involved in locomotion would have been recovered by learning, while the easier task of performing, while at rest, simple isolated movements with a comparatively few muscles remained impossible. Such a conclusion would be directly contradictory to conditions in man (Scherb, 1928a, b). It seems just as likely that the incoordination merely became more obvious in specialized movements, and that it was also present but not detected in other movements. Spitzy's less detailed account of recovery after partial crossing of the obturator and femoral nerves in the dog is even less convincing.

Trick movements and other sources of error in estimating recovery of muscle coordination. It is clear that past reports of good recovery of normal muscle coordination following experimental interchange of hind limb nerves have been generally invalidated by the erroneous assumption that ability to use a limb in seemingly normal fashion implies the presence of normal muscular coordination and coincidentally by the failure to present convincing evidence that the recoveries actually involved an adaptive shift in the function of the crossed nerves. As already mentioned, there are a number of deceptive factors other than recovery of normal muscle coordination which may tend to restore the utility of a limb and hence must constantly be watched for in estimating the quality of motor recovery.

The majority of the items tending to cause errors of judgment as to recovery of motor function have been included under the heading of "trick movements," otherwise referred to as "compensatory," "supplementary," "vicarious," "substitutionary," "accessory," or "anomalous" muscle function. Trick movements have long been recognized (Pitres, 1916; Hughes, 1918; Woods, 1919; Pollock, 1919, 1922; Jones, 1919; Coleman, 1920; Ingham,

1920; Forrester-Brown, 1920b; and others) as a frequent misleading symptom of the utmost importance in the diagnosis and treatment of peripheral nerve wounds.

The methods most commonly cited by which trick movements can be produced are the following: (1) Muscles supposed to be supplied only by a nerve which has been completely divided may continue to function in whole or in part by virtue of anomalous nerve supply. Practical experience with peripheral nerve injuries has taught that the classical standardized descriptions of muscular innervations are not to be relied upon in diagnosis of nerve injury and recovery (see Highet, 1943). (2) Remaining sound muscles, or combinations of these, whose action resembles that of the affected muscles can often, with or without practice, be made to reproduce the actions previously performed by the affected muscles. (3) The resiliency of tissues holding a joint often permits movements to be made in the direction of the action of the affected muscles merely by relaxation of sound antagonist muscles. The release of active contraction of the antagonists is followed by a passive rebound of the limb segment to or beyond the resting position. (4) Owing to the mechanical relations of tendons and ligaments of the limbs, displacement of one joint may frequently cause a passive movement of another joint farther distal or proximal. This passive mechanical effect is due primarily to the presence of muscles or tendons stretching across more than one joint. (5) Movements, particularly of the heavier limb segments, may be produced by gravity. Extension, for example, may be obtained simply by allowing a limb segment to fall from the flexed position, or vice versa, depending on initial posture. (6) Movements, of the distal joints particularly, may be produced by momentum. The distal segments may be flung by more proximal segments in flail-like fashion into a desired position. (7) Movements may be produced by pushing against or pulling on, or otherwise using an outside object such as chair, desk, floor, or examining table as a fulcrum or other mechanical aid to effect a movement no longer possible with the affected muscles alone. (8) At joints used primarily for stability and support or where rigidity is more beneficial than a loose paralysis, recovery may be affected by processes of fibrosis, contracture, or even ankylosis of the joint. Compared with the initial flaccid paralysis, the resultant steadiness may bring about a great improvement in the general utility of the limb. (9) The action of healthy antagonists may be suppressed to restore a more favorable balance of power and so effect improvement that might erroneously be attributed to recovery in the paralyzed muscles.

By any or all of the above means, separately or in combination, a patient may, with continued practice and in the complete absence of any functional recovery of the divided nerves, come to perform with considerable smoothness and little apparent difficulty or hesitancy movements which in the period immediately following nerve division seemed impossible. Most of these factors contributing to trick movements can, by appropriate measures, be eliminated in clinical examinations, and in increasing degree this is being done routinely in the pre-operative diagnosis of nerve lesions. It has not been the practice, however, to take similar precautions in anywhere near equal measure in judging the degree of functional recovery after nerve suture and regeneration. On the contrary, only the most general description of functional improvement has usually been given. One reads, for example, that the patient was able after recovery to play his banjo, ride a bicycle, or return to his job, or the operated dogs could run and jump, could hardly be held back with a leash, or could be exercised on the street without attracting attention. Marble, Hamlin, and Watkins (1942), among others, justifiably deplore the general lack and inconsistency of criteria of recovery which have existed in the past, indicating that it has been the greatest stumbling block in the study of nerve repair, whereupon they offer as a new standard for estimating recovery another vague and unanalytical criterion, namely, "a hand which would prove useful for ordinary occupation and average everyday life." Establishment of a truly fundamental and useful body of knowledge from which generalizations and predictions can be made regarding the possibilities and limitations of motor recovery after nerve regeneration must be founded upon much more specific and basic criteria of recovery, expressed in terms of the contraction patterns of the affected muscles themselves.

It remains to be pointed out that the foregoing list applies to cases in which no reinnervation of the paralyzed parts has occurred. The problem becomes more complicated, and additional misleading factors must be taken into account, in analyzing coordination after reinnervation has been established. A number of the above possibilities for producing trick movements are considerably

enhanced by the return to the paralyzed muscles of contractile strength, tonus, or the active contracture which often follows reinnervation. When the recovered movements can be definitely ascribed to the reinnervated muscles there is still the question of whether the timing of the motor impulses involved has actually undergone any adaptive modification. Correct movements may be due to the firing of nerve fibers whose original muscles contracted synchronously with the new muscles. The synergism and antagonism of muscles is a complicated matter; many so-called antagonists actually show co-contraction in a large proportion of movements. The lack of thorough knowledge of muscle kinesiology in man except for the simplest of movements, is a drawback in interpreting the results of nerve crossing, particularly in the limb.

Clinical

By the old method of lateral implantation the tibial nerve has been made in man to supply both its own musculature plus that of the divided peroneal. And contrariwise the peroneal nerve has been made to supply its own muscles plus the antagonistic muscles of the tibial nerve (Spitzy, 1907). Also the sciatic nerve, after severe partial degeneration, has been cut or crushed in order that the remaining healthy fibers might reinnervate through multiple branching an additional supply of new end-organs (Dogliotti, 1935; and others). Implantations resulting in partial crossing have also been made between obturator and femoral nerves in man (Spitzy, 1905, 1907), and Maragliano (1911) crossed a branch of the crural nerve of the left leg contralaterally to the distal end of the entire crural nerve of the right leg. The reports of recovery in these and similar cases, as well as in those more numerous cases in which severed nerves of the leg have been reunited to their own stumps, imply that some improvement in function was obtained in a majority of cases from the operations. On the whole, however, recovery in man has definitely been disappointing compared to the complete success frequently reported in animal experiments. This is usually attributed to the fact that optimum conditions for nerve regeneration prevail in experimental nerve suture, whereas quite the opposite is apt to be true in the repair of accidental nerve lesions. Failures, when mentioned, have usually been attributed to factors preventing complete nerve regeneration.

Those accounts, on the other hand, in which good recovery of function was said to have been achieved have not been adequate for use as evidence of central reorganization after nerve regeneration, for the following reasons: (a) Recovery has been judged frequently merely by the increase in size of the reinnervated muscles, by the muscular contractions elicitable by electric stimulation, or by the patient's ability, on command, to bring into contraction the reinnervated muscles along with others of the region, with no regard whatever for the differential coordination of the reinnervated muscles in natural movements. In cases where natural activities have been considered the use of the affected parts is described vaguely as "improved," "able to walk without clumsy appliances," and in other such non-specific terms. (b) The normal contraction phase of various muscles in the highly differentiated movements of man is difficult to ascertain without special methods and has not been worked out except for the simplest of movements. It is thus difficult to know in many cases whether recovery of adaptive function of an abnormally innervated muscle in the performances described implies readjustment in the function of the dislocated nerve or not. (c) Although most of the clinical reports have devoted much space to methods of exposing, handling, and reuniting the nerves, the description of surgical procedure generally does not include those details most important for evaluating the functional results, such as the exact level of suture with respect to the inclusion or omission of various nerve branches, the muscles affected and those not affected, and similar details. (d) Reciprocal crosses are, of course, not undertaken, and if nerve crossing is used at all, attempts are made so far as possible to use nerves synergic in function which subsequently require no reeducation. (e) Control cases and anatomical checks after observation have been lacking. (f) Practical difficulties in following up the later results of operations have seriously detracted from the completeness of reports of recovery in man. (g) Instead of being careful to prevent regeneration of the original nerve connections as in experimental preparations, any measures possible have naturally been taken to promote such regeneration. (h) In the great majority of reports of recovery after regeneration of limb nerves, the nerves have been reunited to their own stumps, which permits branching of single axons to several of any of the affected muscles. It cannot be determined under such conditions whether a failure to achieve dis-

sociated muscle action has been due to axon branching or to central nervous implasticity. Nor can it be known to what extent any successful recovery may have been due not to reeducation but to chance termination of fibers on appropriate end-organs. (i) Surgeons have naturally been more inclined to report their apparent successes than their failures. Sargent (1920) mentions that a man examining his own cases could not help being prejudiced in favor of seeing good results. Any such bias becomes especially important when there are no standardized objective criteria to limit and control description of the results. (j) Where the question has arisen as to whether reeducation under given postoperative conditions would be possible, it has generally been presumed that the answer is affirmative, and as evidence have been cited most often, not clinical results, but the old animal experiments of Flourens, Rawa, Kennedy, Osborne and Kilvington, and others. (k) That the occurrence of central readjustment cannot safely be inferred from the uncritical clinical accounts of "good recovery" is further emphasized by the fact that, even more often than in experimental studies, excellent recovery has been reported within periods from a few hours to a month after operation (Foerster, 1918; Perthes, 1922), well before the divided nerves could possibly have regenerated and restored function to the paralyzed muscles. Such obvious mistakes were no longer made, of course, after it had become more generally appreciated that it takes considerably longer for divided nerves to reestablish functional relations. Although errors of this kind have since been restricted to reasonable time limits they have not been eliminated. Coleman (1941), for example, mentions that he has frequently been impressed in delayed exploratory operations by finding nerves still completely divided when repeated clinical examinations had appeared to justify the assumption that these nerves were recovering their function spontaneously.

Results on Forelimb

Particularly in man, but also in the lower vertebrates, possibilities for reeducation would appear to be more favorable in the case of the forelimb than in the hind limb. The greater variety and differentiation of forelimb movements, the greater influence of the cortico-spinal system on forelimb movement, the larger cortical representation of the forelimb, the increased degree to which forelimb movement is subject to visual control, the less automatic or more voluntary nature of forelimb movement, and, in the quadrupeds especially, the more direct effect of forelimb movement on vestibular and visual orientation and equilibrium would all presumably favor the forelimb in the detection and correction of errors in motor coordination. Scherb (1938), in discussing central nervous reorganization after muscle transplantation in man, emphasizes that there is a decided difference between the upper and lower limbs and that reeducation is much easier and becomes more complete after transplantation of arm muscles than of leg muscles.

Experimental

The great majority of previous workers who have studied the effect of crossing forelimb nerves experimentally have not reported any permanent discoordination. On the contrary, in the forelimb as in the older hind limb studies, it has been maintained with few exceptions that a good recovery of normal coordination follows nerve crossing. Most of the experimental work has been carried out on the dog, and the nerves usually selected for crossing have been the large trunks of the radial, median, and ulnar nerves, and occasionally the musculocutaneous nerve. These nerves supply all muscles below the elbow. They also give off branches high in the brachium to the muscles of the upper arm, but in most of the experiments the nerves have been cut and crossed in the middle of the brachium or near the elbow, leaving intact all branches to the upper arm muscles. The operations have been designed to test the effect of interchanging in various ways the innervation of the extensor muscles of the forearm, supplied by the radial nerve, and the antagonist flexors, supplied by the median and ulnar nerves.

Reciprocal crosses between the median and radial nerves in the dog were carried out by Stefani (1886). He reported a recovery of voluntary, coordinated movements, ability to hold a bone or give the paw, although the nerves for flexion had come to serve for extension and vice versa. Gunn (1886) crossed the central end of the divided median nerve to the distal ends of both radial and ulnar nerves in the dog. Subsequently a perfect condition of motility was observed. Time and practice, according to him, may bring order out of the confusion created by abnormal innervation.

Cunningham (1898) made reciprocal crosses in

the dog between the radial nerve supplying extensor muscles and the median and ulnar nerves supplying flexors. He found incoordinations amounting in some activities almost to a reversal of normal movement. These incoordinations persisted for 15 months without any correction. On the basis of his results he flatly contradicted the previous conclusions that correct function is recovered after nerve crossing. Later Kennedy (1901) repeated nerve crosses in the dog similar to those of Cunningham, except that he included with the median and ulnar the musculocutaneous nerve in order to be certain that there would be no muscles remaining below the elbow with innervation intact. Contrary to Cunningham, he found a complete return of coordinated movements. Coordination of the operated limb after recovery was said to be perfect in walking, running, jumping, and in running rapidly up and down stairs. The paw was also used correctly in isolated movements of the forelimb.

Satisfied that it had been demonstrated that nerves crossed to antagonist muscles could give up their old function and take on a new function suited to the antagonist muscles, Kennedy (1914a, b) undertook experiments to find out if nerves were capable of mediating both their old function and at the same time a new function also, i.e., if a nerve could serve correctly both its original muscle group plus the antagonist group. He considered inconclusive the older experiments of Kilvington (1905) on this question, cited above. He cut all four nerves of the forelimb and connected the distal ends either to the central stump of the radial nerve or to the central stumps of the median, ulnar, and musculocutaneous nerves in order .hat all muscles below the elbow would be supplied in some animals from extensor centers only and in other cases from flexor centers only. He again reported good recovery of coordination and concluded that in the limb of the dog, when the nerve supply of one group of muscles has been eliminated, the nerve of its antagonistic group may be used to supply both groups and that under these conditions coordinated movements may be restored. Thus, with only one exception, it has been concluded from the above group of experiments that nerve regeneration to foreign muscles in the forelimb of the dog is followed by a recovery of normal coordination.

The accounts of good recovery are subject to criticisms similar to those raised against the reports of complete recovery in the hind limb. Only a small number of animals was employed in each study. The results were highly variable, with often as many failures as successes. The failures were attributed to contractures, faulty regeneration, stretching of ligaments, etc., and were discounted. As an index of recovered muscle coordination, the investigators in all cases relied upon the animal's ability to use the operated limb in various uncritical activities. The muscles of the proximal joints of the limb, where most movement occurs in locomotion and general activities, retained their normal innervation. Recovery of good use of the limb in these cases was dependent, not upon restoration of any refined differential or reciprocal movements, but merely upon the maintenance of a steady supporting position of one distal joint, the carpal joint. With the paw placed palmar surface down on the ground, the body weight tends to extend, i.e., dorsi-flex, the carpal joint passively. Over-extension of the joint, however, is prevented by the mechanical arrangement of bones and ligaments, so that much support is obtained in the extended position without any muscular activity at all. The extent to which muscular action can be dispensed with and not seriously affect use of this joint is illustrated by the following: Howell and Huber (1892), after crossing the median and ulnar nerves in the dog, noted that on the second day after the operation, with both median and ulnar nerves cut and crossed on the left side high in the arm and also with the ulnar cut on the right side at the elbow, there was little evidence of paralysis or even awkwardness. Before the end of the first week the closest scrutiny could detect no abnormality of movement, except possibly in running upstairs, and it was not certain whether this was due to over-zealousness or to the unusual innervation. Cunningham (1898) also crossed the ulnar and median nerves in the dog in preliminary experiments, and found that 4 days after the operation one could hardly see any difference in the forelimb movements. Gunn (1886) excised a section of the ulnar trunk and could find no impairment of motion in the parts supplied by the ulnar; and in his nerve cross experiments he reported that even the parts supplied by the median nerve, whose distal stump had been excluded from the cross-sutures, recovered from their paralysis. Thus according to observations of the type on which the conclusions regarding recovery were based, both the large

median and ulnar nerves and their muscles comprising the greater mass of the forearm musculature can be completely dispensed with and cause no noticeable defect in the dog's forelimb coordinations. Some function of the radial nerve is of aid in maintaining sufficient extension so that the paw lands on the ground palm down, but according to Osborne and Kilvington (1910a) good recovery may be effected in the complete absence of this extensor nerve also, even when the lack of muscle balance remains unremedied with the antagonist flexors intact.

It is probable that the natural mechanical stability of the carpal joint demonstrated by the foregoing would be further enhanced by the trophic changes in the muscles and connecting and supporting tissues, particularly with the use of corrective plaster casts, as in Kennedy's experiments. One would hardly anticipate a pure reversal of movement as a result of crossing these large compound nerves supplying the entire array of antibrachial and intrinsic paw muscles. The return of tonus and mass contraction to the reinnervated muscles would additionally increase the joint's stability, provided an overbalanced flexor contraction was prevented in the early stages of recovery. The role of complementary muscle action in the more proximal joints and in more widespread regions of the intact system, in covering up and minimizing any slight defects that were present, would also aid the appearance of recovery.

Stefani reported signs of recovery as early as the 30th day after crossing the median and radial nerves; Gunn found paralysis disappearing in less than 4 weeks; and Kennedy obtained a complete return of coordinated movements, as exhibited in walking and running, as early as the 32nd day after cutting and crossing all four major trunks of the forelimb at the level of the elbow. That the nerves had regenerated and the reinnervated muscles begun to function effectively at such early dates appears extremely doubtful (cf. Gutmann, 1942).

All the foregoing discussion, indicating the unreliability of the conclusions that central readjustment has followed the crossing of forelimb nerves in the dog, is supported further by the opposing conclusion of Cunningham, who used a larger number of animals and who appears to have attempted a more careful analysis of both the anatomical and functional results of his operations.

Contralateral crosses between nerves of the left and right forelimb affecting muscles acting on the elbow as well as on the carpal joint were made by Osborne and Kilvington (1910a). In one dog they sutured the proximal stump of the left radial nerve cut as far peripherally as possible to the distal stump of the right radial nerve cut as far centrally as possible. In a second dog they attempted to suture one of the brachial cords to the whole right plexus, but subsequent dissection revealed that only an anterior portion of the right plexus had been included. On examination ten months after operation, coordination, as demonstrated by ability to walk and run naturally and to use either forepaw to hold down a bone, was found to be very good in both animals. It is perhaps more surprising that in this experiment recovery was obtained on the left side, where the nerve was removed without substitution, than that it occurred on the right side with crossed innervation. Apparent recovery on the left side at the same time renders meaningless their inferences about recovery of coordination in the right limb and discredits their conclusion that the experiments demonstrate that an interchange of function can readily occur in the motor centers of the cerebral cortex. The utter unreliability of most of these early uncritical accounts of recovery after nerve crossing is obvious.

Perhaps the most striking recovery observed to follow the crossing of limb nerves is that reported more recently by Barron (1931–1934) after anastomoses between the fore and hind limb nerves of the rat. Accepting the earlier reports of complete recovery of normal motor coordination after interchange of spinal nerves, Barron made nerve crosses for the purpose of studying particularly the role played in the "relearning" process by the sensory fibers of the redistributed nerves. He crossed the median and ulnar nerves of the forelimb into the hind limb, uniting them to the distal end of the divided sciatic nerve in some cases and in others to the distal end of the divided femoral. He also made crosses from the hind limb into the forelimb by uniting the central end of the posterior tibial nerve to the peripheral end of the median and ulnar nerves. Results were similar in the different types of cases.

Nerve regeneration was at least partially successful, and recovery of muscle function in the region of the redistributed nerves was reported in 18 of 37 cases. Contractions of the abnormally innervated muscles were at first always associated with movements of the limb in which the nerves were originally distributed. These associated move-

ments in time became partially dissociated in 10 of the 18 cases, and in 4 of these 10 animals the dissociation of movement became complete. In these 4 cases, after relearning had occurred, it was impossible to get any associated movements whatever even by violent stimulation. The movements of the limbs were reported to be completely coordinated within themselves and also normal for their position. Suspecting the number of sensory fibers in a regenerated nerve to be an important factor in this relearning, Barron effected partial deafferentation of the limb from which the redistributed nerves originated. This was done in 6 additional animals prepared especially for the purpose, and in all cases there followed, as anticipated, a decrease in the degree of dissociation previously achieved by learning, which in 3 of the 6 extra cases had gone to completion. From these results it was concluded that associated movements after nerve crossing can be avoided by using nerves which have a large proportion of sensory fibers.

In order to connect fore and hind limb nerves, the central stump must be cut far distally and the peripheral stump far proximally. Therefore fibers from a few small muscles of the distal segment of the original limb are forced to supply many muscles both large and small distributed over several segments of the foreign limb. That recovery of normal coordination by learning could occur under these conditions would indeed be remarkable. It is particularly significant, from both the practical and theoretical viewpoints, that the relearned coordinations should persist without relapses even in sudden reflex reactions to violent painful stimulation. Learning of new motor habits can hardly reach a more complete stage.

Unfortunately, lack of detail in describing the responses precludes consideration of alternative interpretations and obliges one to take as they are given the general statements of complete recovery. It can only be pointed out that the observations and conclusions remain unreconciled at present with other more rigidly controlled results obtained after nerve crossing and muscle transposition in the rat (see below), in which it was found that under conditions decidedly more amenable and conducive to reeducation than those prevailing in the foregoing experiments the rat failed completely to restore any semblance of normal coordination. It would appear, according to Thorburn (1920a), Buzzard (1921), Ford and Woodhall (1938), Bristow (1941) and others, that even man lacks the extreme reeducative capacities ascribed to the rat in the above report.

To obtain a simple situation easy to correct by learning, Sperry (1942a) crossed in the rat the nerve of the biceps flexor muscle of the upper arm to the nerve of the antagonistic long triceps extensor muscle. All other brachial muscles acting on the elbow were excised, leaving in the upper arm only the triceps muscle innervated by the biceps nerve. In addition, the contralateral forelimb, ipsilateral hind foot, and the tail were amputated in order to make the animals more dependent on the operated limb. Control animals were similarly prepared, with the triceps muscle retaining its own innervation. In order to support themselves on the test limb with the dexterity of the control cases, the experimental animals (seven cases) had merely to learn to excite the biceps motor neurons when normally they would activate the triceps neurons. The nerves of only one muscle were involved; no reciprocal adjustment was required; and this type of nerve operation, like muscle transposition, did not necessitate the extreme dissociation and reintegration of functional properties at the cellular level which must follow the random regeneration of heterogeneous nerve trunks. Nevertheless, the crossed nerves continued to discharge in their original flexor phase of limb movement, regardless of the fact that this now produced elbow extension instead of flexion. The animals never learned to use the reinnervated triceps muscle in the extensor phase to support themselves on the operated limb like the controls. Action potential analysis thirteen months after operation showed clearly that the extensor muscle was still contracting without correction in the flexor phase of locomotion and of other movements. Operations in which nerve crossing was combined with muscle transplantation to produce a reversal of elbow movement in both directions, requiring a more complicated type of adjustment, were also followed by permanent discoordination without any corrective adjustment in the contraction of the reinnervated muscles (ten cases).

Clinical

Improvement in function has been reported in man after partial nerve crossing produced by the method of implantation or by direct neurotization of paralyzed muscles at various levels of the arm from the brachial plexus to the distal nerve branches of the finger muscles (see Sherren, 1906b; von Hacker, 1914; von Hofmeister, 1915; Köllicker,

1917; and others). In some cases at least (Sick and Saenger, 1897; Spitzy, 1907, 1908; Hayward, 1917), the authors were convinced that the observed improvement involved readjustment in the central associations of the crossed nerves. These early clinical accounts are uncritical and in general open to censure on the same grounds as those of recovery in the leg. There is therefore little use in speculation regarding the authenticity of the central readjustments inferred. The more recent reports of functional recovery after nerve regeneration in the limbs of human patients are more analytical in some cases. On the whole, they tend to contradict the optimistic conclusions of the early workers. They deal with recovery, however, not after nerve crossing or lateral implantation, but after straight reunion of the ends of the same nerve, and hence their discussion is referred below to the separate treatment of that subject.

Results in the Region of the Facial Nerve

The reports on the results of nerve crossing and nerve grafting for correction of facial paralysis provide by far the most reliable and most numerous data on motor recovery after nerve regeneration in man. The clinical accounts of the quality of functional recovery in the face may be accepted with much greater confidence than reports of recovery in the limbs. Some of the reasons for the superiority of the records of recovery in the region of the facial nerve may be listed as follows: (a) Defects in the quality of the recovered function show up more strikingly in the face than in the limbs and are usually of greater concern to both patient and surgeon. This has focused more attention on the *quality* of functional recovery in the face. (b) Facial movement is a much more delicate indicator of underlying muscle coordination than is limb movement, because the facial muscles are inserted to the skin and other lightweight, easily movable structures. In the face the slightest contractions and twitches register an observable effect. (c) The inaccessibility of the central end of the facial nerve in the temporal bone, and other factors, have fostered the practice of crossing foreign nerves to supply the paralyzed muscles to a much greater extent in the face than in the limbs. The advantages of nerve crosses over straight reunion of a single nerve, for determining the possibilities of central readaptation after foreign innervation, have been mentioned. (d) The right-left symmetry of facial movements and the close proximity of the control reactions on the normal side of the face make comparison between operated and normal sides easier, more accurate, and also more valid than comparisons of recovered limb movements with contralateral or other controls. For the same reason, the establishment of criteria of recovery presents no such problem in the face as it does in the limbs. (e) The greater frequency of facial nerve paralysis and the earlier application of nerve crossing methods for treatment have furnished a longer and more extensive clinical background of experience. (f) Not only is there more material on which to base conclusions, but this material, due to standard procedures of operation, is much more uniform and comparable than is that on recovery in the limbs. (g) Normal recovery cannot be approximated by mass contractions, contractures, fibrosis, etc. Recovery at all approaching normal function demands differentiated contraction of the affected muscles in distinct patterns. (h) Errors of interpretation due to anomalous innervation, not uncommon in the limbs, are largely eliminated in the face. (i) The possibility of executing trick movements with the aid of gravity or inertia is obviously greatly reduced. (j) In the face, there is much less chance for remaining healthy muscles to simulate the action of the affected muscles. (k) Finally, but by no means least important, is the fact that any observer will have become accustomed through life-long experience to reading the subtlest variations of facial expression and may therefore without special training be relied upon to detect even the slightest distortion or defect of coordination, which is not at all true in the case of limb movement.

For the above reasons the present status of our knowledge of motor recovery after nerve regeneration in man is based largely upon the results of reinnervation of the facial muscles. The experimental observations are closely associated and in the main consistent with the clinical records, so that the two may be considered together. The large numbers of recorded cases prohibit separate consideration of individual results but permit a more summarized discussion.

The nerves first tried and ever since most commonly employed as substitutes for the paralyzed facial nerve have been the spinal accessory and hypoglossal. Early in the history of these nerve cross operations, it was noticed that after reinnervation by the spinal accessory nerve of the shoulder the facial muscles tended to contract in association with shoulder movements rather than

with the normal facial expressions of the opposite side. Similarly, when the hypoglossal nerve of the tongue was used, contraction of the facial muscles became associated with tongue and chewing movements. Thus it soon became obvious that the transplanted nerves retained the central timing which was suited to their normal end-organs, instead of automatically taking on new functions adapted to their new end-organs. Among the early reports (Sherren, 1906b; Kennedy, 1911a), it was claimed in a small minority of cases that no such associated movements developed, but these claims have not been confirmed. By now it has become generally agreed that associated movements must be accepted as an inevitable result, occurring invariably in all successful cases, after crossing the hypoglossal, spinal accessory, glossopharyngeal, or other somatic motor nerves to the facial nerve (Ballance, 1923–1932; Ballance and Duel, 1932; Duel, 1932, 1933; Ford and Woodhall, 1938; Coleman, 1940).

There remains the question of whether or not these abnormal associated movements can eventually be corrected by reeducation. Some of the early reports were optimistic with regard to this point. Kennedy (1911a, b), for example, claimed restoration of voluntary dissociated face movements in dogs, monkeys, and in human patients after crossing the hypoglossal or spinal accessory nerves to the facial nerve. He reported recovery not only of voluntary closure of the eyelids but also of reflex blinking in a human patient, and also in animals in which the reflex persisted under light anesthesia. Frazier (1924) has pointed out that the recovery of these subconscious reflex reactions in Kennedy's cases is indicative of much more complete and stable central reorganization than is recovery merely of voluntary movements.

It is significant that these early reports of recovery of normal facial movement were based largely on the restoration of eyelid movements. Closure of the eylids closely simulating normal reactions may frequently be observed in persons with complete facial paralysis. These "trick" movements of the eyelids may be produced by relaxation of the levator muscles of the upper lid aided by retraction of the eyball (Cushing, 1903; Kennedy, 1911a) or, according to Bender (1936), by active contraction of the orbicularis muscle itself through an extra nerve supply via the levator branch of the oculomotor nerve. Whatever their mechanism, the presence of these movements in complete facial paralysis destroys confidence in those assumptions that the recovery of lid closure responses involved any readaptation in the function of the crossed nerves. Recovery of complete tonus or slight contracture in the orbicularis muscle, after nerve regeneration, as well as improvement in the "trick" mechanism through increase in strength of the muscles concerned and through greater control over their contraction, could lead to gradual improvement in function over a period of time which might easily be mistaken for reeducation involving the function of the crossed nerve itself.

It is generally agreed now that the early reports were inclined to overemphasize the few meager signs of good recovery that could be found and to omit mention of the obvious shortcomings of the results. Although optimism regarding the possibility of restoring normal facial movements with practice has been expressed more recently in isolated instances (e.g., Foerster, 1930; Phillips, 1938; Goldstein, 1939), the prevailing opinions at present are less hopeful. The most extensive investigations of the late results of facial nerve substitution have been carried out by Ballance. One of the first to attempt a clinical application of nerve crossing, he soon became displeased with the hypoglossal or spinal accessory crosses because of the associated movements which regularly resulted. The associated movements were so distressing to the patients and persisted so tenaciously as to mar seriously the effect of the operative treatment. Consequently he and his collaborators conducted extensive experiments on more than a hundred baboons, monkeys, dogs, and cats, trying successively all the additional nerves of the neck which could be crossed to the facial nerve, hoping to find one which would produce satisfactory motor recovery with little or no associated contraction (Ballance, 1924–1932). Many of these varied nerve crosses were tried also on human patients. As a result of their extensive experiences, it was finally concluded that the best method of treatment was to avoid nerve crosses entirely and to attempt the more difficult procedure of repairing in the bony canal the damaged facial nerve itself. In one of his latest reports (1932), Ballance stated that we do not know whether the associated movements produced by crossing the facial nerve ever disappear or not. Certainly they regularly failed to disappear after several years in the experimental animals, and their persistence in human patients for many years without any appreciable improvement has been well authenticated.

At the same time there is no question but that some voluntary control of facial movement can be acquired after nerve crossing. It is easy for a patient after crossed nerve regeneration deliberately to lift his shoulder or roll his tongue in order to elicit contractions in the face. To what degree the facial movements can be dissociated by practice is still controversial. It would be expected that, if the muscles supplied by the 11th or 12th nerves could voluntarily be contracted separately under normal conditions, dissociation should be possible as well after the nerves have been crossed to the face. An appearance of gradual dissociation of function is occasionally mentioned. The reports are not at all critical, however, and do not make it clear whether the dissociation is real, or only apparent and achieved by the patient's learning to conceal or counteract with antagonistic muscles the overt movements of the tongue or shoulder. The extent to which voluntary movements, whether dissociated or not, may eventually become automatic and adaptive is another unanswered question. Kilvington (1941) believes that these voluntary movements are possible only when the patient makes a mental effort to move the tongue or the shoulder at the time when facial movement is desired (see also Coleman, 1940). Attempts to imitate facial expression by this means, it is widely agreed, never reach the point where the facial movements become at all natural in appearance. They are always executed with an obvious impairment of spontaneity. Sudden or emotional movement on the normal side of the face is accompanied only by an appearance of full paralysis on the affected side (Phillips, 1938; Kilvington, 1941). Moreover, the facial movements show no variety of expression; the reinnervated muscles always contract en masse (Coleman, 1940) or in abnormal stereotyped patterns, depending on the nerve used and accidents of shunting in nerve regeneration.

Some improvement in the patient's appearance may result from training by the cultivation of a "dead-pan" expression. Both sides of the face then take on a blank expression, and the discrepancy between the normal and affected sides of the face becomes less apparent. This learning to inhibit facial expression is of course quite a different thing from learning to make the deranged nerve connections of the affected side subserve normal function to match that of the normal side. Not infrequently, in the limbs as well as in the face, no movement at all is better than discoordinated movement, and improvement in function may therefore be achieved simply by dropping out the function of the parts involved. For practical as well as theoretical purposes, this negative type of improvement in function after nerve crossing should always be distinguished from any positive central nervous reorganization. Associated movements may also be avoided in a similar negative fashion by deliberate inhibition of the action of the parts originally supplied by the substitute nerve. It has now become customary to instruct patients to practise not positive readjustment, but rather suppression of all facial movements (Coleman, 1940; Cooksey, 1941; Collier, 1941). This is an outcome of a growing conviction that it is impossible to achieve by reeducation any material success in simulating normal facial movements with substitute nerves. Furthermore it has been found that attempts at positive reeducation, far from improving the patient's coordination, will often increase his deformity by exaggerating the facial grimaces and causing them to become habitual.

In summary, the results of crossing the facial nerve in experimental animals and in human patients indicate that very little central nervous readaptation occurs. The reports in recent years have tended more and more to admit the inadequacy of what few voluntary movements are regained, to refer to the permanent persistence of associated movements, and to state definitely that even in the most successful cases there is always a complete lack of any recovery of natural emotional expression. It must not be assumed, however, that this acknowledgment of the failure of the crossed nerves to give up their original central associations and take over new ones suited to their new terminations implies that such nerve cross operations are no longer justified. It is true that whenever possible it is much better to restore function by reuniting the two ends of the facial nerve itself, but when this is impossible, or when it is a matter of relieving facial spasm, nerve crossing may still be indicated because the restoration of muscle volume on the affected side of the face in itself produces a worth-while improvement in facial appearance, particularly when the features are in in repose (Coleman, 1940, 1944b).

Mass movements after nerve crossing

In addition to the association of facial movements with those of the shoulder or tongue after crossing the spinal accessory or hypoglossal nerves to the facial, there is also an association of movements within the face itself. Contraction of one

part of the face is associated with contraction of other parts, so that individual movements of the eyelids, lips, or brow on the affected side are impossible. The entire reinnervated musculature reacts much as a unit (Coleman, 1940; and others). This undissociated contraction of the facial muscles after nerve crossing, referred to as "mass movements," "contractions en masse," or "synkinesia," may also be considered a type of associated movement, although of a different order from that which occurs between the face and the shoulder or tongue muscles. Wherever nerves are crossed there arises the problem in recovery of dissociating function not only between the new and old regions supplied by the crossed nerve but also between the various reinnervated end-organs within the new region of distribution.

Such mass contraction or associated movements within the face are due to the fact that the motoneurons of the substitute nerve which previously functioned together continue to do so after being redistributed haphazardly throughout the face, instead of becoming individuated in function according to the different muscles in which they now happen to terminate. Another factor conducive to these mass movements is the multiple branching of regenerating axons, resulting in the linkage of single axons to several different muscles. For recovery of normal function within the face it would be necessary for the original functional organization of the rerouted motoneurons to be completely broken down and the neurons regrouped into new motor pool arrangements according to their common peripheral terminations. Those neurons with branches into more than one muscle would have to be inhibited in all reactions where the different muscles did not function synergically.

This problem of dissociation within the area of redistribution of a crossed nerve has been given little or no direct investigation after nerve crossing in the limbs, either experimental or clinical, although in many of the reports (e.g., Barron, 1934; Bethe and Fischer, 1931) it has been assumed that this type of dissociation had occurred. Estimating the extent of functional dissociation between the new and old areas of supply of a crossed nerve has itself been difficult enough in the limbs; to judge the finer degrees of dissociation within the area supplied by a single nerve would be much more difficult. In the face, however, it is easier to tell whether or not the function of the various muscles is properly individuated, and some evidence on the matter is available.

Undissociated mass movements of the face have regularly been reported after crossing the hypoglossal or spinal accessory nerves to the facial. Cases of which it has been possible to report that these mass movements have gradually given way to individuated function as a result of practice have been rare. These few observations can be accounted for on the basis of (1) trick movements of the eyelids, and (2) incomplete spotty reinnervation of the group of facial muscles and threshold effects, such that with low intensity of contraction only one or a few muscles produce noticeable movements. Dissociation of mass movements within a region supplied by a crossed nerve is presumably more difficult to achieve by learning than is the more gross type of dissociation between the new and old regions of nerve supply. The bulk of evidence is clearly against the possibility of dissociating mass movements in the face. There is no reason for supposing such dissociation to be any more feasible after the crossing of limb nerves.

Mass movements after straight nerve reunion

Since the demonstration by Ballance and Duel (1932) that one can often repair the paralyzed facial nerve by grafting in the facial canal and can thereby avoid the troublesome associated movements that follow nerve crossing, attention has shifted considerably from the problem of reeducation after nerve crossing to that following reunion of the stumps of the facial nerve itself. Although the haphazard shunting of regenerating fibers in the nerve scar after reunion of the two ends of the same nerve creates anatomical confusion and poses a problem of central nervous readjustment, the required adjustment is often of a somewhat different order from that following nerve crossing and has special aspects which deserve separate consideration. Discussion of motor recovery after straight reunion of nerves has been delayed to this point because conditions in the facial nerve and its muscles are particularly favorable for illustrating the problems involved.

There is a great range in the degree to which facial nerve regeneration may result in the formation of atypical nerve connections. In cases where paralysis is caused merely by compression of the nerve, there may be no intermixing or shunting of fibers, so that regeneration leads to restoration of the original terminal connections. Increasing degrees of fiber shunting result from increasingly severe lesions due to infections and other factors. In some cases, therefore, none or only a few of the

regenerating fibers may be misdirected, while in other cases of severe infection or complete traumatic severance of the nerve the majority of fibers may be redistributed to foreign muscles. Especially after complete nerve division and the insertion of a graft into the nerve gap, regeneration results in a highly random rearrangement of the normal nerve connections. Since the problem of central reorganization is more important, and the adaptation capacities are more crucially tested in the latter type of case, the following discussion is limited to complete nerve lesions with extensive shunting of fibers into abnormal channels.

Three types of functional disorder have commonly been recorded after regeneration of the completely disrupted facial nerve. First, mass contractions, i.e., inability to individualize the contraction of the various muscle groups. Different facial expressions or separate movement of the lips and eyelids are impossible; the patient can only contract en masse the entire musculature of the affected side of the face—except for the frontalis muscle, which for some reason usually fails to recover any function. Second, facial tics, purposeless brief twitchings of the reinnervated muscles. These tics may be confined to a few muscles or may be present in most of the reinnervated region. Third, contracture of the facial musculature. All the recovered muscles tend to show a sustained active contracture which in some cases may result gradually in a permanent anatomical shortening of the muscles. The contractures become more pronounced when facial expression and tonus is heightened on the normal side of the face.

All three of the above functional disturbances may be attributed to the same fundamental cause, namely, the persistence without central adaptation of the original functional properties of the various facial axons after they have branched and become redistributed at random throughout the array of facial muscles. When, for example, isolated movement of the lips is attempted, the axons originally terminating in the lip muscles are activated, but because these axons have been widely redistributed among the facial muscles there results, instead of separate lip movement, a mass contraction throughout the affected half of the face. When automatic blinking movements of the lids on the affected side would occur normally, the brief flicks of muscle contraction, instead of being restricted to the eyelid muscle, appear in other muscles about the face and are called facial tics. Whenever any muscle would normally be activated all muscles tend to be activated. Every muscle is thus kept in a prolonged state of contraction throughout those periods when any muscle at all would be active under normal conditions. This means that individual muscles get much less chance for complete relaxation and are kept instead in a persistent state of contraction most of the time. Contrasting sharply with these abnormal results of extensive fiber shunting in regeneration are the qualitatively excellent recoveries which follow regeneration after paralyses due to compression in which there is no misdirection of the regenerating fibers.

The mass contractions, facial tics, and contracture, according to most recent reports, are not corrected to any appreciable extent by reeducation. Howe, Tower, and Duel (1937) could see no abatement of tic movements in 27 monkeys with regenerated facial nerves over a period of 2 years. Coleman (1944b) states that normal facial activity is never restored. Ford and Woodhall (1938) cite the case of a patient told to practice before a mirror, in which seven years later all the abnormal phenomena were still present. The history of this case was said to be typical of at least a hundred similar ones in their records. These authors believe that the above phenomena are inevitable in all cases of severe facial palsy and that they persist throughout the rest of the patient's life.

On the other hand, some observers have expressed belief that their patients showed at least slight improvement with practice. Martin (1940), for example, inferred from the improvement observed in two of his cases that the facial tics would probably disappear in a few years. Where functional improvement has thus been reported, however, it is not at all clear whether a positive specific readjustment in the function of the misdirected nerves was indicated or whether the adjustment was simply inhibitory in nature, due to the generalized suppression of the facial centers. Central inhibition of the blinking mechanisms would, of course, eliminate the facial tics. Similarly the complete suppression of all facial excitations should eliminate all three abnormal motor phenomena. The importance of distinguishing between such inhibitory adjustments and positive adaptations of function must be reiterated.

According to more recent views (Collier, 1941; Cooksey, 1941), it is best for patients with motor deformities following facial nerve regeneration to cultivate by practice a "poker face," just as after facial nerve crosses, for there is little or no chance of achieving reeducative adjustments of the posi-

tive type. To what extent the mass movements, tics, and contractures can be eliminated by generalized inhibition is still not clear. It is thought at present that attempts to practice positive readjustments usually result only in accentuating the abnormalities. Therefore the reports like those of Ford and Woodhall that describe permanent persistence of the abnormal phenomena may not be applicable to cases instructed from the start to practice inhibition of facial movement rather than positive corrections. For prognosis based on previous case results, it is important to know whether the earlier patients had practiced suppression or positive correction of facial movements.

The permanent persistence of abnormal motor phenomena and the absence of recovery of normal movement after straight reunion are not necessarily proof of the lack of adaptation capacity in the nerve centers. They could be attributed also to excessive branching and misdirection of individual axons, for, so far as is known, dissociation of the action of individual branches of the same motor axon is impossible in mammals by any amount of reeducation. If only a small percentage of axons had branches to separated muscle groups these axons could be inhibited, theoretically, and a good functional result obtained by using only those axons whose branches terminated in the same or synergic muscles. But if nearly all axons had supernumerary branches to separated asynergic muscles, dissociation regardless of central plasticity would be precluded by the peripheral conditions. It must therefore be remembered that lack of dissociation after straight reunion of nerves or lack of dissociation within the area supplied by any crossed nerve may be attributed theoretically to peripheral axon branching as well as to lack of central adaptability.

In this connection it has been demonstrated that a great deal of axon branching may occur in regeneration of the facial nerve (Lipschitz, 1907; Howe, Tower, and Duel, 1937; Ford and Woodhall, 1938), but there have been no pertinent estimations of its proportions in connection with the problem of central reorganization. Howe, Tower, and Duel (1937) attributed the common abnormal motor results of facial nerve regeneration primarily to axon branching and suggested that reeducation within the facial nucleus probably would correct what abnormalities might otherwise result from fiber misdirection without branching. Collier (1941) is of the opinion that normal dissociated movements after recovery tend to predominate over the abnormal mass movements. This might be taken as a sign in favor of the idea that reeducation corrects the function of all but those fibers with supernumerary branches. On the other hand, it might mean merely that redistribution of regenerated fibers has not been entirely random, that due to incompleteness of the nerve lesions, good orientation of the nerve stumps, or possibly unknown factors favoring retention and maturation of normal over abnormal connections there has been a statistical predominance of normal terminations. Spiller (1919) and Ford (1933) ascribed mass movements and contracture to fiber misdirection alone, with no emphasis on axon branching. Pertinent information on this issue could be obtained by crossing cleanly two branches of the facial nerve. This should produce associated movements within the face, caused by misdirection of facial fibers, but without axon branching between the two muscle groups involved. If readjustment within the facial nucleus is possible, it should occur under these conditions. Such an experiment was undertaken by Fowler (1939). He crossed reciprocally in a "series" of monkeys, two branches of the facial nerve, one supplying the lower lip and the other the eyelid muscle. The signs of partial denervation gradually disappeared completely. No tic or associated movements developed. However, no anatomical check was given to determine whether the cross-sutures had been successful. In two other monkeys the same nerve branches were crossed only one way, that to the eye being crossed to the distal stump of the nerve to the lips. One-way crosses of this sort are technically much easier, and the chances of the sutures being successful are greatly increased. Also, reeducation under these conditions would presumably be less difficult, since only a simple one-way adjustment is required instead of a reciprocal rearrangement. In both of these two animals, however, nerve regeneration resulted in associated tic-like movements of the lip synchronous with eyelid movements on the normal side. Once established, this abnormal "blinking" of the lip remained constant over 8 months, as long as the animals were observed. In these animals lip movement of normal character had already returned in as early as 10 days after nerve section, and the eyes could again be completely closed after 4 months, although no new innervation to the eyelid muscles had been supplied. Since both functions were recovered, without reinnervation, the recoveries cited above after reciprocal crosses may similarly have occurred independently of any crossed innervation. In contradiction to the more

reliable portion of his own evidence, Fowler oddly drew the conclusion that when nerve bundles are rerouted, reeducation takes place so that no association or tic-like movement occurs. He maintained that tic-like and associated movements are caused not by improper rerouting of nerve branches but by splitting of axons in the neuroma. Clearly the experiment should be repeated.

It may be said in summary that the question of how much the lack of positive reeducative adjustment after facial nerve regeneration is attributable to axon branching and how much to lack of central plasticity remains unanswered. This problem is of more than theoretical interest. From the standpoint of the micro-mechanics of nerve regeneration and repair, the branching and the misdirection of fibers are two different things. It might well be possible, for example, in many cases to reduce axon branching to a minimum even though considerable fiber misdirection be inevitable. This would be a worthy achievement, if it were certain that unbranched misdirected fibers are easily adapted in function to suit abnormal terminations.

The oculomotor nerve supplies an array of muscles the differential function of which, as in the case of the facial muscles, is relatively easy to analyze. Functional recovery after regeneration of the oculomotor nerve has been described by Bender and Fulton (1938) in a chimpanzee, and by Bender and Alpert (1937), Bielschowsky (1940) and others in man. Undifferentiated mass action of all muscles supplied by the nerve has been observed just as after facial nerve regeneration, and has been attributed to the same cause, namely, the disorderly regeneration of axis cylinders into the distal nerve stump and consequent disarrangement of normal terminations. The chimpanzee was not retained to find out if reeducative correction could be achieved, but in a human patient Bender and Alpert found all the characteristic motor abnormalities still present seven years after the paralysis had been inflicted. In general nature, the immediate and late results of oculomotor nerve regeneration have been described as being identical with those of facial nerve regeneration.

Motor abnormalities after straight nerve reunion are not so easily detected in the limbs, for reasons already stressed. In many of the older clinical accounts of recovery after straight nerve regeneration, it was assumed that reeducation could and did smooth out the motor deficiencies which otherwise should follow the misdirection of motor fibers. There has been a growing tendency, however, to acknowledge more fully the qualitative shortcomings of nerve repair, and there seems, on the basis of the more recent descriptions, little reason to believe that the late functional results of straight nerve regeneration in the limbs are essentially different from those obtained after regeneration of the facial and oculomotor nerves. The summary reports of Swan (1918), Thorburn, Sargent, Forrester-Brown, and Joyce (Thorburn, 1920a, b), Buzzard (1921), and Bristow (1941), among others, indicate that qualitative motor defects due to fiber branching and shunting in straight regeneration of limb nerves in man are of regular occurrence. Mass action and the development of permanent contractures have been described. It is commonly recognized that recovery is best after regeneration of nerves whose fibers are relatively homogeneous in function, like the radial nerve. Lack of readjustment would, of course, be the less noticeable, the more homogeneous the functional content of the nerve. Recovery is poorest after regeneration of nerves like the median, which supplies many small muscles that normally function in complex differentiated combinations and in which the failure to recover dissociated action produces a distinct handicap. In the more heterogeneous nerves there is a marked contrast between the qualitatively excellent recovery that follows regeneration after mere compression with no shunting, and the qualitatively poor recovery that follows full severance, even though in the latter case regeneration may be quantitatively good (Buzzard, 1921; Gutmann and Guttmann, 1942; Berry, Grundfest, and Hinsey, 1944). It is not infrequently acknowledged that the more refined and complicated coordinations are never recovered after regeneration of severed limb nerves. Ford and Woodhall (1938) describe persistent contraction en masse of the muscles supplied by the ulnar nerve five years after its regeneration in man. Although recovery was quantitatively good, sufficient dissociation of muscle function to fasten buttons was still unattainable. The picture of the results of nerve regeneration in the limbs is on the whole definitely less clear and less well authenticated than that for the face, but the foregoing suggests that the possibilities and limitations of reeducation are not essentially different in the limbs.

Recovery after severance and regeneration of a nerve is therefore probably never complete in a qualitative sense, due to fiber misdirection. Complete recovery could be expected only if all the nerve fibers were strictly homogeneous, but even

in the radial nerve this is far from being true. It is further questionable that even the somatic motor fibers of a nerve branch to a single simple-type muscle are truly homogeneous. There are suggestions from the histological and physiological work on muscle and its motor units (Denny-Brown, 1929; and others) that different fibers may have different properties with respect to the size of the motor unit supplied, its location in the muscle, the central threshold of excitation at which it fires, the frequency range of discharge, the type of muscle fibers supplied with respect to their speed of contraction, and their resistance to fatigue. Thus it may be that the somatic motor fiber supply of a simple muscle is quite heterogeneous when examined in detail, and that accordingly the normal function of the muscle depends upon some orderliness in the termination of the different motor axon types within the muscle. Random redistribution of the motor axons of a single muscle would not be expected to lead to any gross temporal displacement of the action phase of the muscle, but one would predict disturbance in the control of fine gradations of intensity of contraction, disturbance of the incidence of discharge for low intensities, decreased strength, and decreased resistance to fatigue. Similar disturbances would, of course, be expected also in increased degree after regeneration of nerves containing several muscular branches. Such motor defects have not been searched for particularly. It is occasionally mentioned (Perthes, 1922; Stracker, 1919; and others), however, that reinnervated muscles remain far below normal both in strength and in resistance to fatigue, although they have recovered approximately their original volume. To what extent such disturbances may be indirectly caused also by misdirection of sympathetic or sensory fibers is not known.

Comparison of results in face and limbs

Others have been impressed by the pronounced contrast between the persistent abnormal mass and associated movements regularly following nerve regeneration in the face and the excellent recoveries of normal function often reported in the limbs, particularly after experimental nerve crossing. The contrast is all the more striking when it is remembered that in man the motivation for correction and concentrated efforts at reeducation of facial distortions has been much stronger than with disturbance of limb coordination. Although recent observations (Ford and Woodhall, 1938; Sperry, 1940–1943; Watrous and Olmsted, 1941; Obrador, 1942a, b) have tended to erase the contrast, it still requires comment. Barron (1934) attempted to account for the difference on the basis of the smaller ratio of sensory fibers in the cranial nerves. Other hypotheses could easily be advanced, but the evidence when weighed suggests that the discrepancy between functional readaptation in the face and in the limbs is probably in large measure more apparent than real, that the motor abnormalities have been essentially the same in both regions but that in the limbs they have been better concealed and compensated for by the subjects, and less easily detected by the observer, less attentively and less accurately recorded.

Results of Crosses Involving Miscellaneous Somatic Motor Nerves

There are a few reports on the crossing of somatic motor nerves not included in the limbs or as substitutes for the facial nerve.

Rawa (1885) crossed the central end of the divided hypoglossal nerve of the tongue to the distal end of the divided vagus in a variety of mammals. After allowing time for recovery he transected the other vagus nerve. Although most of the animals died shortly afterward, as if both vagi had been cut, survival was sufficiently long in some cases to suggest to Rawa that the crossed hypoglossus was transmitting effective impulses. The absence of any check, to insure that the central vagus stump had not reestablished connections, or that merely a recovery of volume and tonus in the laryngeal muscles or atrophy of these muscles (Schafer, 1919) would not in itself prolong survival, renders these cases inconclusive.

Calugareanu and Henri (1901) crossed the central end of the hypoglossal motor nerve in the dog to the distal end of the lingual nerve of the tongue, in which nerve run autonomic efferent fibers to the salivary glands. After regeneration, salivation on the operated side was about five times as profuse as on the normal side. Since both hypoglossal and salivary fibers are excited together during eating, no temporal disarrangement was to be expected. No adaptation in the intensity of reaction was mentioned. One-third of the hypoglossal was sutured to the distal end of the chorda tympani by Glasson (Anokhin, 1935) in dogs, but the quantity of salivation after recovery in these cases was never as great as normal.

Erlanger (1905) crossed the hypoglossal nerve, and in other cases a stump of the brachial plexus, to the distal end of the vagus in dogs. He found no

evidence of any restitution of normal innervation of any of the organs to which the vagus is distributed except the heart. He observed in one or two of his five dogs some reflex effects on heart rate associated with respiration and with electrical stimulation of afferent nerves, and he concluded that regenerated fibers of spinal nerves may serve as the efferent path of cardiac reflexes associated with the act of respiration and of reflexes started by electrical stimulation of afferent nerves; and that through these fibers the central nervous system may exercise a tonic inhibitory control over the heart. In one case he felt that there was some evidence that the inhibitory center of the heart had actually shifted to the nucleus of origin of the crossed spinal nerve. The evidence in his cases was admittedly extremely meager, and the few results recorded do not necessarily indicate any shift of central nervous integration.

The anterior root of the phrenic nerve was crossed to the distal stump of the cervical sympathetic trunk in cats by Cannon, Binger, and Fitz (1914, 1916). This was done for the purpose of delivering a volley of impulses to the superior cervical ganglion every time the animals breathed. Four animals which survived the regeneration period developed marked symptoms characteristic of exophthalmic goiter in man, including tachycardia, hypernormal metabolism, hypernormal excitability, respiratory hippus, exophthalmus, loose bowels, falling hair, dilatation of pupil on the operated side, and hyperplasia of the adrenal glands. The animals died within three months of the first appearance of the symptoms, except for one case in which the thyroid gland on the affected side was removed. This animal lived normally until purposely killed seven months after the thyroidectomy. It may be said that the animals died because the discharge of the crossed phrenic nerve fibers failed to become adaptively modified to suit the new peripheral connections. Attempts by others to obtain these results (see Burget, 1917; Marine, Rogoff, and Stewart, 1917), however, have been unsuccessful.

Efforts have been made to restore function to paralyzed vocal cords by substituting a nearby healthy nerve for the paralyzed recurrent laryngeal nerve (Hoessly, 1916; Colledge, 1925; Blalock and Crowe, 1926; Colledge and Ballance, 1927, 1928). The central stump of the vagus, phrenic, or descendens hypoglossi nerves has been united to the distal end of the divided recurrent laryngeal nerve in experimental studies on goats, dogs, monkeys, and baboons, and Hoessly inserted a branch of the spinal accessory directly into the laryngeal muscles in the dog. Because of the location of the vocal cords, observation of natural movements has been restricted to the rhythmic adduction and abduction of the vocal cords in tranquil breathing under light anesthesia. When the intrinsic laryngeal muscles were reinnervated by the vagus, spinal accessory, or descending hypoglossal nerve, the tone of the muscles was restored, effecting some improvement over the paralyzed condition, but there was no recovery of normal respiratory movements (Hoessly, 1916; Colledge, 1925; Colledge and Ballance, 1927). Colledge attributed this to the lack of an adjustmental shift in the central timing of the discharges of the crossed nerves. He suggested that if the vocal cords could be observed under other conditions, one would find abnormal associated movements of the cords, for example, during swallowing after use of the descending hypoglossal. When the phrenic nerve was used, rhythmic movements of the cords in quiet respiration were often recovered, but this is because the phrenic impulses are normally associated in breathing with those of the recurrent laryngeal nerve, so that no central nervous dissociation was required. The timing was not perfectly accurate after reinnervation by the phrenic, however, and sometimes incoordination amounting to a complete reversal of abductor and adductor movements resulted. When the animals became excited, incoordination produced stridulent respiration. There was no indication that such incoordination of the automatic movements of the vocal cords supplied by the phrenic nerve was ever corrected by reeducation.

One might expect that reeducation would be easier in the case of the more voluntary movements of phonation in human patients. In 10 patients in which satisfactory anastomosis of the descending hypoglossal to the recurrent laryngeal was performed, Frazier and Mosser (1926) describe the results as "failures" in 4 cases, "improved" in 5, and 1 (unilateral paralysis) "recovered." Judging from the conspicuous avoidance of the problem of recovery of motor coordination in this report after its previous anticipatory introduction (Frazier, 1924), and from the references to this work by Colledge (1925) and Ballance (1924), it appears that the results were not satisfying in this regard. Another case in which the phrenic was tried is also mentioned, without any details of the pattern of the recovered laryngeal movements.

Ballance (1924) reported a surprising instance in

which normal contractions of the diaphragm were restored after its reinnervation by the descending hypoglossal nerve, but he adds a cautionary footnote saying the observation needs confirmation by further experiment.

With the specific purpose of testing the readaptation capacity of the central nervous system, Cordero and Carlson (1927) crossed the anteriormost root of the phrenic nerve to the distal end of the nerve to the sternohyoid muscle in 5 dogs. Regeneration was successful in 4 cases, with the result that the sternohyoid muscle showed contractions synchronous with each inspiratory act and proportional in intensity to the depth of respiration. These contractions were very weak and could not be distinguished until the muscle was exposed through the skin. There was nothing to indicate any shift in the respiratory center toward modifying or dividing its discharges so as to eliminate the abnormal, superfluous contractions of the sternohyoid. The animals were kept three to six months after operation, but no sign of readjustment was noted in any of them.

Results of Crossing Autonomic Nerves

When the central stump of an autonomic nerve (Ranson's terminology) is made to reinnervate foreign end-organs by nerve crossings, readjustment frequently necessitates reorganization of central nervous patterns which normally are of an involuntary or automatic nature. It may be difficult or impossible to separate so-called voluntary and involuntary activities; nevertheless, there are extremes of behavior to which the terms have been applied, and the possibility of there being a difference between them in reeducability after nerve crossing is worth noting. It is commonly assumed that the greater the degree to which a given reaction or activity is subject to voluntary control, the easier should be reeducative correction after crossing nerves mediating the activity. The glandular and smooth muscle responses governed through the autonomic system are in general less subject to voluntary regulation than are skeletal muscle responses, and therefore one would expect to find less evidence of central nervous reorganization after crossing autonomic nerves than after crossing somatic nerves.

Rawa (1885) crossed the central end of the divided vagus nerve to the distal end of the hypoglossal in several different mammals and inferred that normal tongue movements can be effected through the crossed vagus. His experiments have been criticzed on the basis that insufficient care was taken to prevent reestablishment of original connections (Langley and Anderson, 1904b; Erlanger, 1905). Rawa's report induced Schiff (1885) to repeat the experiments. He crossed the central end of the vagus to the hypoglossus in 5 dogs, being careful to excise widely the two uncrossed stumps. No return of normal tongue movement was found. Instead, rhythmic movement correlated with vagal functions began to appear between the 11th and 16th weeks after operation. Reichert (1885) made a thorough examination of Schiff's dogs at six months after operation. At this time there was still no sign of normal movements on the affected side of the tongue; only abnormal tremors associated with various actions of the vagus occurred. Isolated areas of the tongue showed rhythmical contractions, some associated with inspiration, others with expiration. Coughing and deep breathing accentuated these reactions. Other tremor movements were found associated with swallowing and vomiting. Thus the various types of vagus fibers which had made connections with the tongue musculature had retained their original central timing, and there were no indications of any adaptive shift to suit the new peripheral connections. The failure of the intrinsic laryngeal muscles to recover normal function after reinnervation by the vagus has already been mentioned.

Langley (1898b) crossed the vagus nerve to the rostral end of the divided cervical sympathetic trunk in 6 cats. After their recovery, he noticed that whenever the cats were lapping and swallowing milk there was an associated retraction of the nictitating membrane of the eye on the operated side, and also a constriction of the ear arteries and a dilatation of the pupil. On one cat whose anger was easily aroused the nictitating membrane would pass halfway over the eye whenever the animal was teased. The membrane gradually returned to its customary state as the animal became calm. The pupil on the operated side tended to constrict during fits of anger instead of dilating, as did the normal pupil. The foregoing symptoms were observed repeatedly and became more marked with the passage of time. These abnormal associated responses indicate a lack of any central nervous readjustment. They persisted as long as the animals were kept, which, however, was at the most only 123 days. Langley also crossed the lingual nerve containing the vasodilator and secretory fibers of the salivary glands

to the rostral stump of the cervical sympathetic trunk in one cat. He observed, beginning on the 53rd day after operation, that the act of taking milk regularly caused a contraction of the arteries of the ear on the operated side. Nerve impulses normally conducted to the salivary glands had become rerouted to the arteries of the ear. Their timing remained associated with salivation without an adaptive shift to suit the new termination of the fibers, but again the animal was kept only 71 days after operation.

The central end of the vagus nerve in the dog was sutured to nerves of the forelimb, usually the radial, by Anokhin (1935a, b, 1936a) and his coworkers. They reported that after regeneration the forelimb muscles reinnervated by the vagus came to function in coordination with the other forelimb muscles in locomotion as well as in withdrawal responses conditioned to a bell. Moreover, stimulation of a specific area of the motor cortex elicited contractions of the reinnervated muscles along with the intact muscles of the forelimb. It was concluded that the intracentral connections of the vagus nucleus change radically as a result of the anastomosis and somehow enter into association with the forelimb motor centers of the cortex. Published along with this study in the same monograph are later studies, in which the same operation is said to result in contractions of the reinnervated forelimb muscles clearly associated with respiration and deglutition. In fact, the action of the forelimb muscle supplied by the vagus is described as a delicate indicator of the exact state of tonus of the respiratory center. All changes in respiration are very closely reflected in the forelimb muscles, and the phenomena are so illustrative and constant that they were used for lecture demonstrations. Further study revealed that the "breathing and swallowing" movements of the forelimb were mediated entirely through the recurrent laryngeal fibers. The results of crossing the vagus nerves, including the recurrent laryngeal fibers, into the forelimb are thus given on the one hand as demonstrating extensive central nervous readaptation, while in other reports they are said to the contrary to demonstrate with striking clearness complete retention by the crossed motor fibers of their original central relations and functions. Such puzzling contradictions are not rare in Anokhin's monograph.

Ballance (1931) crossed the central end of the divided cervical sympathetic trunk to the distal end of the facial nerve in cats, dogs, and monkeys. He mentioned no abnormal associated movements following these crosses, nor did he mention any voluntary coordinated movements or emotional responses. He also crossed the central cervical sympathetic nerve to the distal hypoglossal nerve but again remained noncommittal about the recovery of coordinated movement, except for the remark that when the mouth was opened the tongue retracted, and both sides appeared to move together.

Although various "trophic" effects due to interruption of autonomic fibers have been described after spinal nerve lesions (Stiles and Forrester-Brown, 1922; Meige and Bénisty, 1916), the problem of the quality of recovery of the different autonomic functions in relation to fiber misdirection remains to be studied.

After regeneration of the facial nerve in man, particularly when the lesion is proximal to the geniculate ganglion, there may result, in addition to the inevitable abnormal mass movements and contracture of the striated musculature of the face, abnormal autonomic phenomena. The syndrome known as "crocodile tears" (Kaminsky, 1920; Kroll, 1929; Ford, 1933) is such. This is excessive lacrimation on the affected side whenever the patient eats or takes any sapid substance into the mouth. Even appetizing odors may cause lacrimation. In other words, whenever salivation occurs, there also results an associated copious flow of tears from the eye on the side of the regenerated nerve. This has been attributed to the aberrant regeneration of salivary secretory fibers into the lacrimal glands of the eye, instead of into the salivary glands where they originally terminated. The central timing of these aberrant fibers remains unadjusted to their new terminations, with the result that tears are produced when salivation is called for. Correction by central nervous readjustment apparently fails to occur (Ford and Woodhall, 1938). Ford (1933) reported a case in which the associated lacrimation had already persisted sixteen years when first observed.

Of similar nature is the "auriculo-temporal syndrome" (Fridberg, 1931; Ford, 1933; Ford and Woodhall, 1938), which not infrequently follows damage and regeneration of the auriculo-temporal branch of the 5th nerve. After nerve regeneration the anesthetic area of skin over the temple comes to show paroxysmal sweating and vasodilation whenever salivation occurs. At other times the

skin in this area is indistinguishable from normal skin. This phenomenon, like "crocodile tears," also has been attributed to the misdirection of salivary nerve fibers. The secretory and vasodilator fibers of the parotid gland apparently misregenerate to the sweat glands and blood vessels of the skin. In these cases also, the profuse sweating and flushing of the temple whenever the patient eats has persisted without signs of correction by central nervous adjustment. A patient in whom localized flushing of the skin with excessive sweating in the region underneath the chin on one side of the throat was always associated with salivation was reported by Uprus, Gaylor, and Carmichael (1934). They ascribed the phenomenon to misregeneration of salivary fibers following an incision in the right side of the neck. The patient was 22 years old at the time of observation and the dysfunction had been present since the age of seven.

It is possible that the abnormal associated phenomena described above were due to the presence of supernumerary branches of single salivary axons, as well as to their misdirection without branching. Even if axon branching were present, however, the distressing phenomena could at least have been inhibited theoretically by complete suppression of discharges to the salivary gland involved or to the salivary glands on the affected side of the face. Such central dissociation of function should be simpler than a dissociation at the cellular level, calling for excitation of some cells of the salivary nucleus with inhibition of those with abnormal peripheral termination. The fact that not even some such generalized inhibition of the salivary nucleus was achieved in the above cases is significant. The best the patients could do, apparently, was to learn to swallow food very quickly or to use food that did not stimulate salivation (Ford, 1933).

To summarize, the observations on functional recovery after misdirection of autonomic motor fibers, except for the report of Anokhin and Ivanow which is contradicted by further studies in the same laboratory, indicate that aberrant motor fibers have continued to function in their original manner and have not acquired, either in man or in animals, new central associations suited to the new end-organs.

FUNCTIONAL RESULTS OF MUSCLE TRANSPOSITION

After the transposition of muscles or their tendons as well as after nerve crossing, readjustment is required in the central timing of motor impulses in order to restore normal muscular coordination. The timing of a muscle's contraction in various movements must of course be adapted to the mechanical action of the muscle. If the mechanical action is changed by transplantation, then the central firing of the motoneurons of the muscle must be shifted to correspond with the normal action phase of whatever muscle the transplant is substituted for; otherwise coordination would be disrupted. The central reorganization required after muscle transposition, however, is usually of a quite different order from that required after nerve regeneration. When a muscle is transposed with its innervation intact, recovery of correct function demands only dissociation and reassociation of functional relations between the intact "motor pool" of the transposed muscle as an organized unit and the remainder of the organized central system. After reunion or crossing of heterogeneous nerves, on the other hand, the haphazard reestablishment of individual nerve connections regardless of previous functional groupings makes necessary a complete breakdown of the original central organization of the elements involved and a thorough reorganization starting at the neuron level. It follows that reeducation should be easier after transposition of muscles than after nerve regeneration.

Transposition of Limb Muscles

Experimental

Manigk (1934) observed that frogs in which the hind legs had been sutured together down to the ankle walked exactly the same after their gastrocnemius muscles had been reciprocally transposed as when these muscles were left uncrossed. He attributed this to instantaneous reorganization of coordination patterns and emphasized the role of peripheral mechanisms in shaping central integration. The experiments were soon repeated and extended by Taylor (1936), who showed that Manigk's conclusions were erroneous because the recovered movements proved to be entirely passive and even persisted after denervation of the crossed muscles. Furthermore, when Taylor made the plantar extensor gastrocnemius muscle into a dorsiflexor in the same leg, reversed movements resulted so that dorsi-flexion occurred when the foot normally would have shown extension. The experiments showed that no immediate spontaneous reorganization follows transposition of these muscles in the frog. The animals were not kept

to find out if any reeducational adjustment might occur with experience and practice. It has been demonstrated by Weiss (1937b), however, that after reorientation and transplantation of entire limbs into such positions that normal coordination is either useless or detrimental, urodele amphibians continue to use the old motor coordination patterns indefinitely without any sign of correction by learning.

Laptev and Anokhin (Anokhin, 1935a), after transplanting part of the lateral quadriceps muscle to a flexor position in the hind leg of the cat, concluded on the basis of the appearance of leg movements in locomotion that the transplanted extensor had changed its function to that of a flexor. When the muscle after 5 months training was isolated for kymograph recording, however, the transplanted portion contracted synergically with its counterpart as a normal extensor. The results were interpreted as being due to a "dynamic rearrangement" in central nervous relations without any fixed change. Because the action of the transplanted portion of the quadriceps in locomotion could easily have been masked by the function of the remaining musculature of the thigh (see J. E. Stewart, 1925; D. Stewart, 1937), the assumption that readjustment had occurred in these cases is not justified.

Transposition to an antagonist position of a dorsi-flexor or a plantar-extensor muscle of the shank in dogs and cats was accomplished by Watrous and Olmsted (1940) in five animals. Their results were the same as Anokhin's. No incoordination in locomotion was evident as a result of the operations, but when the animals were decerebrated and the transplanted muscles were isolated and prepared for kymographic recording, it was found that their reflex activity under these conditions was the same as if transplantation had not been performed. Absence of readjustment after decerebration cannot, of course, be taken as proof that no reeducation had occurred.

It had already been shown (Sperry, 1939; 1940) that in the rat a clear-cut reversal of flexor and extensor movements results after reciprocal transposition of dorsi-flexor and plantar-extensor muscles, provided care is taken to abolish the normal action of associated muscles which might otherwise obscure the action of the transplants. When dorsi-flexion is called for, the transplanted dorsi-flexor contracts producing extension, and vice versa. This definite reversal of foot action was found in all activity, in slow deliberate voluntary movements as well as in reflex responses. Moreover, it persisted without any correction by central reorganization. Control animals in which the nerves of the transposed muscles were also crossed demonstrated that the transposed muscles were quite capable of producing flexion and extension in the correct phase of leg movement if, as was here provided by crossing the nerves also, the central discharges to the muscles were correctly timed. It was obvious that no foot movement at all would have been more advantageous to these animals than the reversed movements and yet, even after amputation of the forelimbs or under special training conditions, the rats not only failed to adapt the central discharge pattern to the rearranged muscle action but failed even to inhibit the maladaptive reversed movements.

It was also found that reversed movements of the elbow in the forelimb of the rat result after transplantation of the flexor and extensor muscles of the upper arm (Sperry, 1942a). Different types of muscle translocation were performed. In some cases the transplantation was made in only one direction, i.e., a flexor was transposed to serve as an extensor, and all other brachial muscles were excised. In other cases reciprocal transposition of flexor and extensor muscles was performed. In a third series, one flexor muscle was left in normal position, and another transposed to act against it. A certain type of trick movement featured prominently in the results, but in none of the different series did the transposed muscles come to take over new function suited to their new mechanical effect. Electromyographic and motion picture analysis revealed that the transposed muscles were still contracting in their original uncorrected action phase after prolonged training under conditions conducive to reeducation.

There is no question but that under these conditions of reciprocal muscle transplantation, where the rat failed to make an adaptive adjustment in the action of the test muscles, man would readily be able to do so in some degree. It would merely be necessary for man to make a mental effort to flex the foot when he wished actually to extend it, and vice versa. To what extent such deliberative corrections could eventually become rapid, automatic, and generalized, so as to transfer readily to unpracticed activities, can only be guessed at present.

Clinical

In clinical practice, clear-cut reciprocal transplantations with excision of other muscles, as in the experimental cases above, are of course never encountered. The problem of functional dissociation and reorganization after transposition of one or two muscles in one direction only, with associated muscles of various functions left intact, as is generally the situation in human patients, is one of greater complexity. Clinical accounts of the functional results of muscle transposition are far too numerous to permit their separate consideration and discussion. As a rule, just as with the observations of recovery after nerve crossing, recovery in man has been viewed from a practical unanalytical standpoint, without much attempt to determine critically the extent to which the central timing of muscles can be shifted to suit new mechanical relations (Leveuf and Perrot, 1937). Opinions which have arisen from general clinical experience are by no means unanimous. Some authors have held that the nerve centers are very plastic and are readily adapted to bring about coordinated movements after antagonistic transplants (Codivilla, 1904; Perthes, 1918; Billington, 1922; Bethe and Fischer, 1931). Other workers have warned specifically against attempts to make muscles serve entirely new and antagonistic functions and state that many failures follow the use of antagonistic muscles for transposition, because of the inability of the transposed muscles to revise their function to suit a new action (Steindler, 1919, 1940; Dunn, 1920; Gill, 1921). The type of transplantation most frequently undertaken in man and most consistently successful is the transposition of forearm flexors to serve as extensors after radial nerve paralysis. The claim that the function of muscles can easily be reversed has been based in no small degree on the results of this classical transplantation to correct wrist drop. It has been pointed out, however (Stiles and Forrester-Brown, 1922; Steindler, 1940), that the transplanted muscles in such cases do not take on an entirely alien function, since the flexor and extensor muscles in this region show a great deal of co-contraction in normal activities. Also there is a pronounced benefit simply from the improvement in muscle balance, so that it is questionable to what extent good recovery in these cases can be considered evidence of central readaptation.

It is generally agreed that muscle transposition very often leads to functional improvement, but this does not imply agreement regarding the effectiveness of reeducation. Stiles and Forrester-Brown (1922) state that although it is possible that an antagonistic muscle can never be trained to work independently of its old group in support of its new one, the results of antagonistic transplantation may nevertheless be beneficial. Improvement in function may be due to a number of factors. Trick movements, improvement in muscle balance about a joint by subtraction from the strong side as well as by addition to the weak side, provision of an elastic opposition for the intact healthy muscles to work against, unspecific stabilizing effect on a joint, natural synergic action of the transplant with the muscle for which it is substituted, among other factors, may all lead to functional improvement in addition to central readaptation in the timing of the transposed muscles. To exactly what extent this latter factor itself can be counted on to aid in recovery is not clear from the varying opinions that have arisen from gross clinical observation.

Even those attempts to approach the problem critically with the use of special methods for detailed analysis of the contraction patterns of the muscles involved have not yielded consistent conclusions. The most persistent effort to determine in a thorough manner the effects on motor coordination of muscle transplantation has been made by Scherb (1928–1938). According to his conclusions, reeducation occurs readily after the transposition of arm muscles. So far as it is surgically possible, any arm muscle may be substituted for any other with the expectation that the function of the transplant can be shifted to suit the new position. In the leg also, complete readjustment is possible, except under certain special conditions as follows: When the paralyzed group of muscles for which an antagonistic muscle is substituted is not completely paralyzed, the remaining functional portion, even though it be the merest remnant, will prevent readjustment in the function of the transplant in automatic walking movements. Adjustment will still be possible in simple voluntary movements, however. Scherb emphasizes strongly the difference between simple voluntary movements which the patient is asked to perform while at rest, and the automatic involuntary act of walking. Ability to use a muscle correctly in a simple deliberate movement is no guarantee that the muscle will also work correctly in involuntary reactions. Scherb also opposes definitely the once common notion that the function of a trans-

planted muscle is shifted immediately and spontaneously in adaptation to its new action, without any training. Considerable practice, he has found, is necessary to reverse the function of a transplanted limb muscle. Scherb's opinions and general rules for muscle transposition, as far as they go, have been widely accepted (Leveuf and Perrot, 1937).

Several investigators have studied experimentally the function of the flexor biceps femoralis muscle after its transplantation to serve as an extensor of the knee in place of the paralyzed quadriceps. In this one case, at least, we have the advantage of independent opinions based on observations in different laboratories. Vinke (1934) studied the action of these muscles in locomotion with the aid of his myokinesiometer. He concluded, on the basis of admittedly few cases, that muscles retain their original or nearly their original contraction phase after transplantation. He pointed out, however, as have also others (Stiles and Forrester-Brown, 1922; Scherb, 1938), that the normal contraction phase of the hamstring muscles is multivalent and is such as to make these muscles of some service in the transplanted position without any change of their normal timing. Some improvement in the use of the limb thus results without any central readjustment. For this same reason Scherb, who also had studied the action of the transplanted hamstrings, did not consider the failure of these particular muscles to give up their original contraction phase as an exception to his principles for functional readjustment. Dunn (1920) mentioned that in his experience transplantation of the biceps to the quadriceps had not resulted in voluntary control of knee extension. Porter (Gill, 1921) also stated that he had never yet seen a transplantation of the hamstrings to replace the quadriceps which was satisfactory. Tubby (1906) and Ritter (1928) were more optimistic in opinion, however, while Weiss and Brown (1941), using electromyographic methods to record action of the transplanted biceps femoris, found a definite adaptive shift in the timing of the transplant from the flexor to the extensor phase in various types of movement. They reported temporary relapses into the old flexor association, however, even years after operation. The above contradictions regarding the possibility of functional readjustment which have arisen about this one type of muscle transplantation that has been most intensively analyzed serves to illustrate the complexity of the problem in man and the lack, as yet, of any complete understanding.

On the whole it may be inferred from the literature that with practice man is capable of dissociating the action of individual muscles from associated muscles so as to adapt the action of a transplant to its new position in at least the simplest voluntary movements. This is easier in the arm than in the leg. The problem after muscle transposition in man is not so much "is any readaptation possible?" as "to what extent is readaptation possible?" Readjustment apparently is not automatic but depends on the learning process, and consequently is more apt to be found in slow, practiced, simple, deliberate, voluntary movements than in rapid, unpracticed, complex, involuntary, surprise reactions. Although most studies and observations have been confined to movements of the former type, some of the studies, cited above, indicate that readaptation of transplanted leg muscles is possible under some conditions in the automatic walking coordinations. The walking coordination recovered by most paralytics after muscle transposition, however, usually resembles little the smooth gait of the normal person, and to picture the transplanted muscles serving in the place of their antagonists in a thoroughly efficient manner in the natural automatic walking movement is to get an erroneous impression of the recovery.

Between simple deliberate movements, on the one hand, and the automatic movements of locomotion on the other, and extending beyond these limits, there is a manifold range of different types of motor coordination, varying greatly with respect to complexity, speed, amount of previous practice, and other factors. The function of transplanted muscles throughout most of this possible range of activity has remained unexamined. That a person after practice could use a transposed flexor to extend his fingers for demonstration does not mean the transplant would work properly in piano playing or even in the many simpler everyday performances which involve habitual and automatic extension of the fingers at the proper time, with the proper speed, and in the proper degree. The extent and ease of readjustment may be expected to vary greatly with different types of performance.

The readjustment problem may be expected to vary also with different muscles transposed, not only with respect to whether they are in arm or leg, but also in accordance with the degree of

functional dissociation to which the muscles are subject under normal conditions and in accordance with the nature of the normal functional interrelations between the transplant and whatever muscle it replaces. Relations of intensity of contraction must be considered, as well as relations of timing. After transplantations involving anatomical antagonists which normally show considerable cocontraction, the required adjustment may be primarily one of intensity. These functional interrelations of muscles with their associated groups and with the group for which they are to substitute will be different for different types of performances, so that these variables are not really separable from those of the preceding paragraph but merely constitute further dimensions in the manifold range of readaptation requirements. In summarizing the literature it can only be said that although some of the reports imply that readaptation, particularly in the arm, may approach completeness, there is as yet very little actual evidence of the extent to which readjustments may go.

Whereas the rat fails to dissociate and readapt as a unit the function of muscles moving one limb joint, man is capable of dissociating and readjusting the function of individual muscles. In regard to this marked human superiority, the great difference between man and the lower mammals in the degree of differentiation of muscular coordination patterns which exists normally is worth noting. In the quadrupeds the limbs are used in a comparatively stereotyped manner. They tend to be flexed or extended as a whole, without differential movements at the various joints. In some cases the limb structure itself tends to prevent differential movements. For example, flexion of one joint at the same time as extension of a neighboring joint may be precluded by the short length of multiarticular muscles and their tendons. Because of the differences between man and the quadrupeds in the normal degree of dissociation of muscle function, the problem of central nervous reorganization after transposition is quite different from the outset. The superiority of the higher forms may be due in large measure to the greater differential control of motor coordination which is present at the start and not entirely to greater physiological plasticity of the nervous tissue *per se*.

The few brief reports of the effect of muscle transposition in the face (Jianu, 1909; Brunner, 1926) for treatment of facial paralysis in man are incomplete and add nothing essential to the above general picture.

Transposition of Ocular Muscles

The foregoing cannot be generalized to apply to recovery after the transposition of ocular muscles, which appears to present special features and has a literature of its own.

Experimental

Marina (1912, 1915) transplanted the insertions of ocular muscles in the monkey and reported an immediate restoration of normal coordinated eye movements in both voluntary and automatic reactions three to four days after operation. On the basis of his results, he concluded that the nuclei and association tracts of the central nervous system have no fixed and predetermined function, as is traditionally supposed. Later Dusser de Barenne and de Kleyn (1928) transposed the external and internal rectus muscles in the rabbit and in 4 out of 8 cases observed normal nystagmus movements of the eyeball as soon as the animals opened their eyes after the operation. They found, however, that the retractor bulbi muscle, present in the rabbit and in most mammals except primates, was capable after extirpation of all six ocular muscles of executing by itself normal horizontal and vertical eye movements. Olmsted, Margutti, and Yanagisawa (1936) transposed the superior rectus muscle to take over the function of the external rectus in dogs and cats. They reported a recovery of normal eye movement as early as three to four days after operation in the best cases. The recovery of coordination was interpreted as being due to a rapid learning process, and it was concluded further that the new central associations were subcortical, because ablation of the eye motor area of the cortex did not abolish the readjustment once it had been established. These conclusions have subsequently been discredited, however, by Watrous and Olmsted (1940), who discovered that the retractor bulbi muscle by itself may in the dog as in the rabbit effect recovery of correct eye movements in all directions. Watrous and Olmsted further transplanted the superior oblique muscle to a point near the internal rectus muscle in rabbits, and recorded the isolated reflex contraction of the transposed muscle following decortication approximately three months after the primary operation. The muscle showed no alteration in its normal timing. This test after decortication did not prove that no

reeducation whatever had occurred, but the results did show that at least no adjustments became so fixed as to become truly reflex.

Although observations of good motor recovery after transposition of eye muscles in lower mammals have thus been discredited as evidence of central nervous readaptation in view of the compensating function of the retractor bulbi muscle, the early results of Marina cannot be discarded on the same basis, for this muscle is so reduced in the monkey that it alone could not possibly effect normal movements in all directions. Bartels (1920) criticized Marina's interpretation, however, on the ground that the action of the remaining healthy muscles would be sufficient to produce the responses Marina observed. He pointed out that a single muscle, the internal rectus, is capable of carrying out nystagmus responses to both sides. He believed all Marina's results could be explained without assuming any central nervous reorganization. In many hundreds of records of isolated eye muscle contractions, Bartels never observed any but standard reactions characteristic of the normal, and he disclaimed any such dynamic plasticity in the coordination centers as suggested by Marina. Bethe and Fischer (1931) supported Marina, nevertheless, on the basis that the records of isolated eye muscles attached to a lever cannot be compared with natural reactions where vision helps to regulate eye muscle coordination.

More extensive experiments on the transposition of the extraocular muscles in the rhesus monkey have been carried out recently by Leinfelder and Black (1941, 1942). They found that coordinated eye movements were recovered as early as eight days after transposing the medial and inferior rectus muscles plus the superior and lateral rectus muscles, leaving the two oblique muscles in normal position. The degree and speed of recovery was not appreciably influenced by placing the animals in complete darkness during the recovery period. The eye movements after recovery were not entirely perfect; a hypertropia was present that increased on looking toward the side operated on; there was restriction of lateral movement; inferolateral movement was absent; and continuous movement of the eyes suggested diplopia. Vertical and medial movements were well executed, however, and the functional disturbances as a whole were considered minor compared to what would be anticipated from the anatomical disarrangement.

When the four rectus muscles were transposed as above and in addition the superior oblique muscle was transected in the same operation, there was then no recovery of coordinated movements. All movements under these conditions were such as should result schematically from the changed position of the muscles. Medial movement of the normal eye was accompanied by upward movement of the eye operated on. When the normal eye looked downward, the eye operated on turned medially. After both eyes had been subjected to the same operation, all movements of both eyes were abnormal, in accordance with the positions of the transposed tendons. These animals were kept four months at least, but there was no sign of any readaptation.

Further experiments revealed that if tenotomy of the superior oblique muscle was delayed following the transposition of the four rectus muscles until recovery of coordination was well established, then the coordinated movements would survive section of the superior oblique. When the superior oblique had already been sectioned in an earlier operation, then coordination was recovered after transposition of the four recti in 2 of 3 cases. Coordination was also recovered when the two pairs of rectus muscles were transposed at separate operations, and the superior oblique sectioned at an intermediate date. The authors assumed tentatively that central nervous reorganization was the most probable explanation of the recovery of coordination. They reasoned that recovery must depend upon visual and proprioceptive cues, and since vision had been eliminated by placing the animals in total darkness, they were inclined to emphasize the rôle of proprioceptive impulses originating possibly in Tenon's capsule.

These experiments, largely confirming and extending the early observations of Marina, would appear to constitute the most outstanding evidence yet presented of radical and rapid central nervous reorganization after muscle transposition. In view of their significance, the experiments deserve special consideration, with thorough and very careful check and control, to be certain that no other interpretation is possible. If it be accepted that central nervous readjustment was actually responsible for the recovery of coordinated movement, then the results are perplexing in several respects. First, if the central integrating mechanisms are as thoroughly plastic and readily adaptable as suggested by the majority of recoveries, why, after certain types of operation should schematic derangement of eye movements have

appeared and persisted permanently with no correction? Why too should the same anatomical result of an operation or a series of operations have been accompanied by recovery of normal coordination in some cases and by appearance of distinctly abnormal movements in other cases, depending only on the order of the operations? That the answer lies in the differential effect on the proprioceptive system is difficult to harmonize with other data on the role of proprioception in reeducation and development of motor coordinations. Proprioceptive cues appear to play a very small role in reeducation after transposition of limb muscles in man, according to Weiss and Brown (1941). Insofar as proprioceptive reflexes result in immediate and almost reflex regulation of muscle response, the proprioceptive discharges would be expected to enhance the original unmodified action of the transplants rather than to aid in adaptation. According to Scherb (1938), proprioceptive impulses from intact associated muscles do inhibit rather than aid reorganization. It is also surprising that the central adjustments were achieved either very early in the first few days after operation or not at all. There was no history of gradual improvement by practice, from abnormal movements to correct rotations. When distinct discoordination was produced by the operations, it persisted. Also counterindicating any kind of learning process is the fact that recovery was not appreciably affected by keeping the animals in total darkness during the recovery period. As contended by Bethe and Fischer (1931), one would expect vision to play a very significant role in any central readjustment of eye muscle coordinations (see also Scherb, 1928b).

Any alternative interpretations ought therefore to be carefully considered. Surgery of the type involved must have presented opportune conditions for formation of scars and adhesions. The tendinous insertions of the transposed pairs of muscles normally border close together. After the muscles had been crossed on each other and reinserted, there would be ample opportunity for the overlapping and coalescence of the tendons and also for the formation of adhesions and extraneous tendon connections to points near their original insertions. Because tendon regeneration and scar and adhesion formations are all profoundly influenced by mechanical tensions, it may be presumed that incipient movements of the eyeball effected during the healing period by the undamaged muscles would be an important factor in shaping the mechanical connective tissue relations between transposed muscles and eyeball. The interrelationship between mechanical stress and connective tissue formation under such conditions is a self-reinforcing one, such that if a particular type of movement once got started, it would influence further structural development in such a way as to increasingly favor the same to the exclusion of other types of movement. The action of the intact muscles would be expected to predominate over that of the freshly transposed muscles, except when the intact muscles were very strongly overbalanced. Early predominance of the intact muscles would be favored further than otherwise appeared by the action of the intact retractor bulbi or accessory lateral rectus muscle, the existence of which was apparently not taken into account. This extra muscle is described by Bast (1933) as a distinct 7th ocular muscle one-fourth to one-third the size of the lateral rectus, occurring only as a rare variation in man but prominent in most rhesus monkeys. One may justly question whether the necessity for leaving two or more muscles intact during the healing period in order to obtain good recovery was not because of the influence of the sound muscles in shaping the connective tissue architecture rather than their influence on proprioception and central readaptation. At present either possibility appears equally plausible. Certainly a very careful dissection of the muscles before sacrifice and a detailed check of the exact mechanical action of each muscle and its parts in reflex responses and in reactions to direct electrical stimulation is needed for an interpretation of the results. The authors themselves were judiciously sparing in interpretation and reluctant to draw any final conclusions. Until anatomical checks and further control experiments are available, it seems advisable to adopt a conservative attitude with regard to the existence of central nervous readaptation in these experiments.

Clinical

According to Bartels (1920), beneficial effects of transposing single eye muscles in man can be accounted for entirely on the basis of passive mechanical effects and other factors such as normal synergic action without assuming any central readjustment of the muscle's activation. Jackson (1923) mentions that the common transposition of slips of two neighboring muscles from either side to compensate for a paralyzed muscle requires

no central adaptation because these are synergic substitutions.

The superior rectus muscle of the eye is sometimes transposed to replace the paralyzed levator muscle of the lid. The upper lid and eye are naturally raised and lowered together in most activity so that no recoordination is required. Attempts to close the eye forcibly after such operations, however, often are attended by a sudden jerking up of the upper lid. This is said to be corrected usually after a few months. Whether readjustment in this instance involves the gradual dissociation and inhibition of the action of the transplant specifically in a particular movement, or whether it involves merely learning to use a different type of movement altogether that from the beginning did not include action of the transplant, is not clear. In sleep the eye rolls up and the lid comes down. Inability to keep the lid closed during sleep, after substitution of the levator by the superior rectus, is not observed, however (Jackson, 1923), and this has suggested to some that adjustment of coordination is involved. But if the position of the eye in sleep is one of muscular relaxation, as commonly stated, there would be no necessity for readjustment.

EFFECTS OF INTERCHANGING SENSORY NERVES

Distortion of sensory as well as of motor innervation follows the reunion or cross union and regeneration of nerves. Because of the greater numbers of sensory fibers in most spinal nerves and because of the greater heterogeneity of function among sensory fibers, misdirection and abnormal termination probably occur on a larger scale with sensory than with motor fibers. In order that the sensations and responses to stimulation of misdirected sensory fibers may be made adaptive, the central nervous associations must be readjusted in accordance with the new peripheral connections. How the required functional readjustments compare essentially with those required after efferent disarrangement cannot be said without more knowledge of the neural basis of readjustment and learning processes in general. The problem of readaptation on the afferent side has thus far received comparatively little attention in either the clinical or experimental studies.

Results in animals

In a number of cases those who have accepted the reports of recovery of normal motor coordination after experimental crossing of mixed nerves have inferred without more direct evidence that central readaptation to abnormal sensory, particularly proprioceptive, reinnervation must also have occurred along with the motor adjustment. In several instances (Kennedy, 1901, 1914a; Barron, 1934; Anokhin, 1935a), the impulses transmitted over the abnormally distributed sensory fibers have been thought to play an important role in the readjustment process. Since, as explained above, recovery of normal motor coordination is in itself very questionable in such cases, the corollary inferences concerning sensory recovery may be disregarded.

Bethe (1905; Bethe and Fischer, 1931) noticed, after crossing the sciatic nerves from the left to the right hind leg in the dog, that although there was no sign of motor discoordination after regeneration, there did appear definite evidence of false localization of sensation. When the right foot was stimulated, the animal raised the left leg and turned to the left side. These erroneous reactions persisted without correction until the animal was sacrificed, more than a year later. Bethe considered the absence of adjustment in this case an outstanding exception to the general rule, however, and expressed the opinion that correction might have occurred if longer time had been allowed.

The posterior tibial branch of the sciatic nerve was crossed into the ipsilateral forelimb in the rat by Barron (1934). After regeneration the animals responded to stimulation of the forelimb by withdrawing the hind limb as well as the forelimb. Contrary to the results observed by Bethe in the dog, these abnormal responses in the rat were soon completely corrected, until stimulation of the area of the redistributed nerves resulted only in normal responses of the limb stimulated. The functional correction was maintained even in reflex responses to violent stimulation. This constitutes another instance in which it has been inferred that processes at the reflex level have been adaptively reorganized by learning. Similar results were described by Barron after the median and ulnar nerves of the forelimb had been crossed to the distal end of the femoral or sciatic nerves of the hind limb. He deduced that not only was function of the abnormally connected afferent fibers corrected in itself, but in addition that these fibers had played an important role in the correction of the motor discoordination resulting directly from the crossing of the efferent fibers. It was concluded that abnormal associated movements can be avoided after

nerve crossing by using nerves with a sufficient number of sensory fibers to be misrouted along with the crossed motor fibers. As already mentioned in citing the motor aspects of these experiments, one can only point out that they are irreconcilable with other observations. The results of crossing sensory fibers observed by Bethe in the dog and more recently by Sperry (1943a) in the rat (see below) are in direct contradiction to these conclusions.

Anokhin and his co-workers (1935a; Anokhin and Iwanow, 1936a) carried out a long series of experiments on the dog in which they crossed the central end of the divided vagus to the peripheral end of various other nerves including the radial median, and subscapular nerves of the forelimb the optic nerve, the lingual nerve of the tongue, and the motor and sensory roots of the 6th and 7th cervical nerves. The experiments were undertaken for the purpose of studying central nervous reintegration, how it occurs, and over how long a period. Upon completion of regeneration after these crosses, abnormal reactions could be elicited from the various regions reinnervated by the vagus. These were most striking and most thoroughly studied after the vagus to radial nerve crosses. Scratching the skin of the shoulder region after regeneration in such cases caused coughing and a guttural rattle. Manipulation of the shoulder muscles caused profuse salivation and vomiting. When the dogs moved about, stimuli from their own activity caused coughing which stopped only when the animals came to rest. Pricking the skin with a pin, and heat or chemical irritation evoked only pain responses. The coughing and guttural rattle reactions were found to be mediated by the fibers of the recurrent laryngeal nerve which were included in the vagus. When these fibers were excluded, the dogs did not cough but still displayed deep anti-peristalsis reactions.

After some months the abnormal reactions began to disappear gradually, until by the sixth to ninth month after operation, strong and continued irritation of the same skin regions on the shoulder produced no abnormal reactions, but only appropriate responses, as after stimulation of normal skin areas. After disappearance of the coughing and vomiting responses, however, prolonged electrical stimulation under light anesthesia of the exposed vagus nerve or the production of an open wound in the skin by a second degree burn lowered the threshold of the abnormal responses so that they reappeared. Slight irritation around the edge of the open wound, for example, would again elicit coughing and vomiting after these could no longer be evoked by the usual means.

The results were taken to demonstrate that the central phylogenetic connections of the vagus nucleus are not at all fixed, but are capable of extensive rearrangement to suit new peripheral connections. The role of the periphery in regulating readaptation of the central associations was emphasized.

That the animals, after disappearance of the abnormal reactions, were able to feel and to localize correctly stimuli applied to the shoulder area of the crossed vagus was to be expected, in view of the extensive overlap in the distribution of cutaneous sensory nerves (Pollock, 1920b; Gutmann and Guttmann, 1942), involvement of subcutaneous and deep sensibility with the stimuli used, and the natural circumferential shrinkage of areas of sensory loss through changes in the neighboring intact nerves (Pollock, 1920b; Weddell, Guttmann, and Gutmann, 1941). It has been reported that due to peripheral anastomoses of nerve branches in the forelimb of the dog, the excision of a single nerve trunk is not attended by a loss of sensation (Gunn, 1886). The conclusion, therefore, that the vagus nucleus had taken on new somatic functions suited to its new peripheral connections in the experiments of Anokhin cannot be accepted in the absence of convincing evidence that the recovered "adequate" responses were not mediated by intact afferent fibers instead of by the crossed vagal fibers.

On the other hand, the gradual disappearance of the incorrect responses seems unequivocal. That the dogs gradually became conditioned so that these stimuli no longer caused coughing and vomiting is not so extraordinary. The type of reorganization involved would presumably be little different from that involved if the animals were repeatedly exposed to natural stimuli conducive to coughing and vomiting, until gradually they became adapted to such conditions. The learning by human patients to swallow stomach tubes without retching and to inhibit overt coughing in certain pulmonary diseases is analogous. Mere inhibition or dropping out of the original reactions after the crossing of sensory nerves does not involve nearly so extensive a reorganization as does the positive reestablishment of appropriate reactions. Gradual adaptive elimination of a direct autonomic reflex like vomiting, however,

is nevertheless of some interest in as low a mammalian form as the dog. That the gradual rise in threshold of the abnormal responses may have been due to depressant metabolic or other local effects of the abnormal nerve terminations rather than to a true learning process is an alternative possibility not excluded.

The nerves of one hind foot were crossed into the contralateral hind foot in the rat by Sperry (1942b, 1943a). All the main nerves of the left foot were crossed to the corresponding nerves of the right foot and the remaining small uncrossed nerve branches to the right foot were excised, so that after regeneration all structures below the ankle on the right side were supplied by nerves that had originally innervated the left foot. The primary operation was performed on young animals only 14 to 26 days of age. After nerve regeneration the animals responded to stimulation of the right foot as if it were the left foot that had been stimulated. This was true even in cases in which the left foot had previously been amputated. There was nothing in the rats' behavior to indicate that they were in any way aware of the illusory nature of these abnormally referred sensations. Painful stimulation of the sole of the right foot caused the animals to withdraw the left leg instead of the right, at the same time to shift the entire weight of the hind quarters onto the right leg, and further to extend the right leg directly against the offending stimulus. These extremely maladaptive reactions, instead of being rapidly corrected after their initial appearance, became more exaggerated as regeneration became complete and thereafter persisted without adjustment as long as the animals were kept, ten and one-half months after operation. Attempts to train the rats by various means to correct the reversed hind limb reflexes were in vain. At the end of ten and one-half months sores in the right foot or painful wire clips pinched into the sole of the right foot caused the animals to walk about on three legs, holding the uninjured left leg in the air and consequently also placing extra pressure on the source of the pain in the right foot.

In contrast to the absence of readjustment of direct reflex responses, the animals did show evidence of learning to locate and remove wire clips pinched on the skin in the area supplied by the redistributed nerves. This apparently did not involve a complete adjustment to the degree that the clips actually felt as if they were on the right foot, for the animals continued commonly to make false turns to the left side and to lick the wrong foot. Nor did this type of learning affect the erroneous reflex withdrawal responses. Even in the act of pulling the clips out of the correct foot, the wrong foot continued to be withdrawn reflexly, and frequently the extra irritation caused by pulling at the clips made the rats abandon the clip on the right foot in order to wheel around and lick the left foot. This persistence of original reflex patterns after adjustment had been made in closely associated activities of a more voluntary nature demonstrates a distinct difference in the susceptibility to reeducative adjustment of different types of central integration patterns. The automatic spinal reactions to strong pain remained quite refractory to readaptation in contrast to the deliberate cortically mediated localizing responses.

Recently Obrador (1942a) tested sensory recovery, as indicated by placing and hopping reactions in cats and dogs, after crossing the peroneal and tibial nerves. These reactions were found to be absent or very defective. Pain sensitivity was roughly recovered, but there was no attempt to test differential function of pain fibers in different parts of the reinnervated areas.

Results in man

Human subjects offer a great advantage for studies of sensory recovery following nerve regeneration, in that they can give direct information regarding their subjective sensory impressions. One would expect the extreme displacement of sensory fibers which must have followed any success with lateral implantation of one nerve into another, as practised in the older clinical nerve surgery, to have resulted in conspicuous distortions of cutaneous localization and other sensory functions. The reports of such partial nerve crosses in man, however, apparently contain no mention of pertinent points on the quality of recovered sensation. Relevant observations of functional recovery after sensory fiber interchange in man are scarce and seem to be based entirely on recoveries following straight reunion of the ends of the same nerve.

General sensory disturbances, such as one would expect to result from chaotic misarrangement of the innervation pattern, have been recorded in some of the more critical examinations of recovery after regeneration of limb nerves in man. Sensory defects in the hand after regeneration of the median and ulnar nerves have attracted particular attention (Sargent, 1920; Stopford, 1930; Ford and

Woodhall, 1938; and others). After regeneration of the ulnar and median nerves, sensations from the affected parts of the hand tend to be diffuse and indistinct. The hand is found to be of slight practical service, even though all the muscles have recovered their volume and contractility. Patients find they lose grip on their tools, the hand fails to function satisfactorily when it is not watched, and is almost useless in the dark or in conditions where it cannot be seen. Generalized pain, tactile, and thermal sensibilities may show good restoration without any corresponding improvement in the refined discriminatory sensory functions. Absent or faulty sense of passive movements of the fingers, uncertainty as to which finger has been touched, gross impairment of two-point discrimination and of stereognosis, and erroneous reports of the mode of stimuli have all been described in cases in which the recovery was said to have been good in a quantitative sense. Head (1920), after prolonged and thorough examination of the affected cutaneous area in his classical experiment, was still making gross errors in the compass test five years after nerve section. The general level of sensory recovery described by Head is rarely approached in clinical cases, according to Stopford (1926), even after primary nerve suture under ideal conditions. Lanier, Carney, and Wilson (1935) stated that the reinnervated cutaneous regions in their experiments never recovered normal sensitivity from the point of view of the character of the effects of areal stimulation. Dallenbach (1931) found it impossible to compute a two-point limen in a reinnervated cutaneous area because of the abnormal disorderly state of the recovered sensibility. Persistence of such general defects as the above for at least three to five years after nerve suture has been recorded in a number of cases, and Ford and Woodhall(1938) doubt that it is possible ever to correct them by reeducation.

More refined analysis of the effects of nerve shunting on recovery of cutaneous sensation is possible in man through the testing of individual sensory "spots." Such tests have been carried out most thoroughly after deliberate experimental section of cutaneous nerves in human subjects. The great diversity in the functional properties of the different sensory fiber types of any large limb nerve makes possible schematically a host of abnormal nerve-end-organ recombinations, following the random redistribution of fibers in regeneration. Two general classes have been recognized by Stopford (1930): (1) Termination of fibers in atypical tissues or end-organs, as when fibers of the articular surface of a joint become misdirected to tactile endings in the skin. Terminations of this first type lead to erroneous impressions of the mode or quality of stimulation. In the given example, tactile stimulation may elicit a sensation not of touch but of movement of a joint. (2) Termination of fibers in the correct type of tissue or end-organ but in a foreign area of the body, as when cutaneous tactile fibers of one finger become misdirected to cutaneous touch endings in a different finger. Tactile stimulation of the reinnervated finger is then perceived as touch but is localized incorrectly to the finger in which the fibers originally terminated. Terminations of this type lead directly to false localization and defects of stereognosis and other functions involving spatial relations. The distinction between these two classes of abnormal sensory termination is of some aid in approaching the problem of recovery, but actually the different possible anatomical recombinations and resulting types of functional confusion are manifold. Lack of knowledge regarding the normal mechanisms and anatomical requirements of sensation, and also regarding the extent to which different fiber types are able to make functional termination in foreign tissues and end-organs, further prevents detailed formulation of the readjustment problem.

Errors of the first type mentioned above, i.e., of the quality or mode of sensations elicited from cutaneous points, have actually been reported in only a few instances. Stopford (1926, 1930) found that pressure stimuli sometimes evoked sensations of burning or of movement at a joint. He suggested that the growth of heat, pain, and posture fibers to tactile end-organs or of pain fibers into heat, cold, or localization pathways would account for these and other odd responses he had observed. Ford and Woodhall (1938) also reported errors of similar nature. In both accounts it has been considered unlikely that this type of functional abnormality could be corrected by reeducation. Whether the failure of most other investigators to notice modal errors of this kind should be attributed to an actual scarcity of the phenomenon, or to the fact that it has not been adequately searched for, remains to be determined.

A persistent generalized intensification of all modes of sensation is apparently conspicuous in reinnervated areas and has been widely reported. There has been little agreement as to its cause

(Trotter and Davies, 1909, 1913; Boring, 1916; Head, 1920; Stopford, 1930; Lanier, Carney and Wilson, 1935), however, and none of the authors have directly ascribed this intensification to fiber misdirection.

The second type of error mentioned above, namely, erroneous localization or false reference of sensations, has been reported commonly in nearly all detailed descriptions of the recovery of cutaneous sensibility after nerve section and regeneration. Sensations of touch, temperature, and pain have been found to be misreferred consistently to various distant points within the region supplied by the regenerating nerve. The sensations are most commonly referred to more distal points, but in the later stages of regeneration reference to proximal and transverse points also occurs. Correctly localized sensations may be present from the start, along with the falsely referred sensations, or may appear in the later stages. A single point stimulus may be referred to two or even three distant points, and Trotter and Davies mention curious instances in which stimuli were localized at the point of nerve section proximal to and outside the cutaneous area supplied by the severed nerve. Stopford, however, said misreference is always to points within the affected area. Trotter and Davies, who apparently paid more attention to the phenomenon of false reference than others, reported that it was one of the earliest and most characteristic accompaniments of recovery and also one of the most persistent. They stated that in some of their cases it remained years after the reinnervated areas had recovered approximately normal sensory acuity. Head, Stopford and others, however, have emphasized that reference disappears with the return of epicritic sensibility. Despite these inconsistencies, all seem to agree that false localization at least tends to give way to correct localization.

The cause of false localization and the reason for its disappearance remain controversial. Langley (1908) and Osborne (1909) believed it was produced by shunting and abnormal termination of afferent fibers, and that its later disappearance was due to readjustment and reeducation of the nerve centers. This view was particularly championed by Stopford (1926) and has since been widely accepted (Lee, 1929; Lanier, Carney, and Wilson, 1935). Boring (1916) accounted for misreference in terms of multiple innervation and the action of inhibitory and secondary fibers. He too believed it disappeared with practice. Head, however, considered misdirection of fibers inadequate as an explanation. Although Trotter and Davies did not contest Langley's hypothesis that misreference is due to misconnections, they, like Head, felt there was more to it. They attributed it primarily to stimulation along their course of newly regenerated hypersensitive fibers, the sensations being referred to the point of the fibers' termination. Gradual return of correct and disappearance of referred sensations they attributed to the formation of end-organ connections and consequent maturation of the fibers. Schafer (1927, 1928) likewise attributed misreference to excitation of fibers along their course instead of at their end-organs. Hoffman (1915) cited cases where peripheral reference of sensations seemed clearly to be caused by stimulation of the outgrowing fibers at their tips or along their course far proximal to any terminations.

That all false reference in the above studies has been due to stimulation of fibers along their course, however, is difficult to reconcile with other facts. Stopford argues, for example, that false localization does not appear in nerve regeneration when a nerve is merely crushed and not severed. Also it has been observed that touch, pain, cold, and warmth all show these abnormal references, and that they are elicited selectively by the correct mode of stimulation. Trotter and Davies therefore inferred that the nerve fibers themselves are selectively sensitive to different modes of stimulation, but Stopford, Schafer, and others have rejected this. Long persistence of false reference when it occurred was attributed by Trotter and Davies to the presence of excess fibers which had not formed end-organ connections and had therefore remained uninsulated and hyperirritable, but as already mentioned, others have denied that false localization persists beyond the first stages of recovery. Trotter and Davies mention that rubbing a point to which itching pain is falsely referred may give considerable relief, even though this point be more than a foot from the stimulated spot. Stopford assumed localization to be mediated by a special set of localization fibers, but others assert that localization is mediated by the regular pain, touch, and temperature fibers (Lanier, Carney and Wilson, 1935). A thorough comparison of the effects of nerve regeneration without shunting, as after crushing or fixing with alcohol, and of regeneration with extensive shunting, as after complete severance, would be extremely helpful on a number of

these problems. In Schafer's study on this plan pertinent points were not recorded, and in the work of Lanier, Carney and Wilson the surgical method of injecting the anesthetic and alcohol into the nerves leaves it uncertain as to whether there may not have been considerable shunting of fibers.

Disregarding the numerous contradictions of the various reported facts and opinions, one gets the general impression that less distortion of sensory mode and localization have been found in these studies than would be predicted schematically on the supposition that the sensory fiber regeneration and termination is random and nonselective. If localization depends upon stimulation of at least two fibers, as inferred by Weddell (1941c), then it is indeed surprising that any sharp localization, correct or false, was recovered at all, since the chances of both members of the correct pairs of fibers terminating together in the same sensory spots would be exceedingly slight on a purely fortuitous basis. Nevertheless, the abnormally referred sensations have been reported as being very distinctly localized with precision at the referred point. This precise but erroneous localization of sensations could hardly be ascribed to learning. In addition, the falsely referred sensations were often accompanied by correctly localized "local" sensations. To what extent these correctly localized sensations in these older experiments were attributable to the overlap of neighboring fibers of cutaneous and deep sensibility is debatable.

Further discussion, in an attempt to arrive at some conclusion from the observations thus far available, is useless. The investigators have disagreed upon too many issues. At the time the studies were made little was known of the anatomy of cutaneous innervation, particularly with regard to nerve overlap and multiple innervation of sensory spots. The complications arising from hypersensitivity in the intermediate zone, and from the sprouting and invasion of collateral fibers (Gutmann and Guttmann, 1942) were not clearly recognized. The experimental approaches have been guided largely by the original ideas and observations of Head, while problems such as those created by nerve fiber shunting have remained unrecognized or at best have received only casual attention. Some critical deductions might be gleaned from these older studies if knowledge of the anatomy and physiology of normal cutaneous sensibility were available as a basis for judgment, but as yet there is still little agreement about even the most fundamental aspects of normal cutaneous sensation (Nafe, 1934; Lewis, 1937; Stone and Jenkins, 1940; Rothman, 1941; Gilmer, 1942a, b; Walshe, 1942; Livingston, 1943). In summary, it is impossible to say from the reports to date whether or not any reeducative central nervous adjustments are involved in the partial recoveries of sensitivity that follow cutaneous nerve regeneration. With the increased knowledge of the distribution and physiology of sensory fibers (see Weddell, 1941b; Tower, 1943; Bishop, 1944, among others) and with the numerous advances in methods for study of the sensory mechanisms, many of these points of confusion left by the older studies might soon be resolved.

TRANSPOSITION OF SENSE ORGANS

The transplantation or grafting of pieces of skin sometimes results in abnormal cutaneous sensation and presents a problem of central nervous readaptation. Generally where a piece of skin is transplanted with most or all of its nerve supply severed, localization within the transplanted area, according to Purdy (1934), is restored correctly by the ingrowth from the surroundings of those fibers which originally supplied the area. One might expect, however, that although localization would be grossly correct under these conditions, discrimination of spatial relations within the graft area itself, and also of the mode of stimulation of individual sensory spots, might be abnormal. Although some observations of recovery of sensation in free skin grafts have been reported (Kredel and Evans, 1933; Davis and Kitlowski, 1934), a thorough, detailed study with the problems of nerve regeneration and the reestablishment of appropriate end-organ relations in mind is still needed and might throw considerable light not only on questions of recovery but also on some of the points of confusion regarding normal sensory function.

When a flap of skin is shifted to a new position with most of its innervation intact, sensations elicited from the transplant are the same as before transplantation, i.e., adapted to the old position instead of the new. For example, Douglas and Lanier (1934) report the results of transposing a flap of skin from the upper to the lower lip, with one end of the flap remaining intact at the corner of the mouth. Stimuli applied to the lower lip were thereafter referred to the upper lip. Such transposition of skin flaps is somewhat analogous

on the sensory side to muscle transposition on the motor side. In both situations there is retention of the original systematic innervation of the transposed part, so that neural interrelations are not thoroughly disrupted at the neuron level as after nerve regeneration. The problem of readjustment is therefore quite different, being presumably much simpler than when the function of individual sensory spots, even of individual sensory fibers, must be corrected and resynthesized into an orderly system. Douglas and Lanier reported that their patient, cited above, gradually learned to localize correctly stimuli applied to the transposed skin on the lower lip. They concluded therefrom that localization depends mainly on habit formation.

Contradictory results were reported by Purdy. He studied localization in a flap of skin from the volar surface of the terminal phalanx of the middle finger which, after accidental amputation of the end of the finger, had been folded over the cut end and sutured to the skin of the dorsal surface with about 6 mm. folded over beyond the end of the finger. The patient was thirteen years old when the transposition was made. Ten years later, stimuli applied to the flap on the dorsal surface were still invariably localized in an illusory fashion to the volar side of the non-existent first phalanx. A single touch at the suture line felt double, one dorsal and the other ventral, with the finger in between, even though the patient was looking at the stimulus. The reports of both Purdy and of Douglas and Lanier fail to distinguish clearly between the patient's immediate subjective impression and his ability to interpret this so as to localize accurately. In Purdy's case it seems clear that the patient could have localized these stimuli correctly by word or gesture despite the illusory subjective impression—obviously so, when he was allowed to watch application of the stimulus. From Douglas and Lanier's report, however, it was not clear whether the stimuli came actually to be felt in the lower lip or whether the patient had simply learned that stimuli which felt to be at one place were really at another. No controls were given that would indicate in any way what actually had been learned by this patient. Also the gradual invasion of fibers from the lower lip into the partially denervated graft was a complicating factor in this case.

The readjustment in the report of Douglas and Lanier was compared to that achieved by Stratton (1896, 1897) after wearing lenses which inverted the appearance of the visual field. Stratton reported that eventually the inverted visual field no longer appeared upside down under certain conditions of relaxed attention, and his results were interpreted by himself and many others as indicating that complete adaptation to visual inversion would occur with prolonged practice. This has been contradicted, however, by the more extensive experiments of Ewert (1930, 1936, 1937), in which 3 subjects wore the inverting lenses more than twice as long as did Stratton, and also by the unpublished study of Peterson (Ewert, 1936), who wore Ewert's apparatus about three times as long as Stratton and who took care to repeat Stratton's procedures. The visual field in all these subjects continued to appear upside down without noticeable change. Directions were seen reversed in movement, and this illusion was so strong it could not be inhibited. Distances tended to appear inverted, and judgment of distance remained poor. A complete adaptation to this sensory inversion with prolonged practice was considered by Ewert to be highly improbable. Nevertheless, certain types of overt localizing reactions and compensatory motor adjustments to the sensory reversal were readily established. These motor adaptations were characterized by typical learning curves.

Comparable visual disorientation effects produced surgically in amphibians have been found to persist indefinitely without correction even of deliberate overt motor reactions (Sperry, 1943b–1945). When the eyeball of an amphibian is rotated on its optic axis through 180 degrees, the animal's behavior thereafter indicates that the visual field is seen upside-down and reversed about the optic axis. Objects above the head are falsely localized to a position below the head, objects in front are localized to the rear, etc. Perception of the direction of movement across the visual field is reversed, as illustrated by optokinetic and pursuit reactions. Persistent circus movements also result. When the eye is transplanted to the contralateral orbit with either the nasotemporal axis or the dorsoventral axis inverted, and the perpendicular axis at the same time normally oriented, then vision after nerve regeneration is correspondingly reversed with respect to the inverted axis of the eye, but normal with respect to the axis correctly oriented. For example, if the dorsoventral axis of the eyeball is inverted, objects in front and above the animal are localized in front and below, whereas if the

nasotemporal axis is inverted, objects in the same position are localized above and to the rear. Also in the latter case horizontal optokinetic reactions are reversed, and continual circus movements are exhibited. When the optic nerves are crossed with each other, then vision after recovery is contralaterally reversed in mirror-image fashion. Objects on the right side of the animal are misreferred to corresponding positions on the left side and vice versa, as indicated by escape, feeding, and optokinetic responses. Perception of the direction of movement in the visual field around the dorsoventral body axis and also around the longitudinal body axis is reversed, as indicated by reversed optokinetic reactions and pronounced circus and swaying movements of the head on these two axes. These maladaptive distortions of visuomotor coordination, diagrammatically correlated with the anatomical sensory recombinations, are apparently not subject in the amphibians to any appreciable correction by central nervous reorganization.

P. T. Young (1928) found that a right-left reversal of auditory localization produced in man by pseudophones persisted without correction after the pseudophones had been worn for 58 hours over a period of 18 consecutive days. Some compensatory adjustments to the auditory reversal were made by increasing dominance of vision over audition and by deliberately turning in the opposite direction from which sounds seemed to come. This, like the results of lenticular inversion of the visual field, is another example of persistence of illusory sensations in man with attendant correction of associated overt responses by learning.

The "phantoms" which appear after various types of amputation have been compared to illusory sensations resulting from nerve redistribution and skin grafting. Phantom limbs sometimes fade out completely in the course of years, but they may also persist permanently without change (Gallinek, 1939; Riddoch, 1941). It is questionable whether much positive central nervous reorganization is involved in the fading of phantom limbs. The process may be entirely inhibitory, rather than reconstructive, and the inhibition may be active or simply a passive dropping out of excitations in the centers involved. Persistent phantoms are usually associated with intractable pain, which apparently prevents fading by keeping the limb centers in a state of high excitability. It may be argued that the failure of the spatial attributes of the sensations to become adapted to the new body form under these conditions is demonstration of a certain lack of plasticity and adaptability in the central associations concerned, but final judgments in this regard had better be postponed until the underlying causes of such phantoms are more thoroughly understood.

The results of skin transplantation, auditory reversal with pseudophones, and lenticular inversion of the visual field in man indicate that motor responses can be readjusted in some degree to suit this kind of sensory alteration, but the problem of reeducation under such conditions is simpler than and must be distinguished from that following random reconnection of individual sensory fibers. To what extent the adjusted responses may eventually become as rapid and automatic as normal reactions remains to be determined. Nor is it clear whether the subjective sensations themselves can ever be corrected by reeducation. The little evidence available indicates not.

SUMMARY AND DISCUSSION

Since the time of Flourens the weight of authority has favored the view that complete or nearly complete functional adaptation after disarrangement of nerves and end organs may be brought about by reintegration of the central nervous relations of the motor or sensory nerves involved. Critical consideration of the evidence, however, reveals little that supports even the more moderate assertions of the past concerning central nervous reintegration. Most of the more remarkable functional recoveries that have been recorded appear to have been cases in which various types of compensatory adjustment on the part of the intact system, along with local mechanical and trophic changes in the affected parts, have together produced a serviceable effect which has been erroneously interpreted to be the product of extreme revision in the central synaptic associations of the affected peripheral nerves. Many factors which may contribute to such spurious recoveries, both motor and sensory, have been cited in the text. Past failure to analyze carefully the various underlying factors contributing to readaptation, and especially failure to distinguish between the indirect compensatory type of adjustment and adjustment in which the function of the affected nerves and end organs themselves is directly involved, has rendered invalid a large percentage of the older conclusions regarding the reintegrative capacities of the nerve centers.

When those observations and experiments in which spurious recovery has not been controlled are ruled out, the capacity of the mammalian nervous system to readapt the function of its disarranged nerves and end organs appears quite limited.

Immediate Spontaneous Reintegration

There remains no convincing support for the idea of recovery by instantaneous dynamic reorganization as postulated by Marina (1912, 1915), Bethe and Fischer (1931), Manigk (1934), and Goldstein (1939). These authors, among others, have asserted that new adequate central coordination patterns emerge immediately in direct response to new peripheral arrangements, without any practice or learning being necessary. Control of central nervous coordination is presumed accordingly to be mainly peripheral and to depend upon some kind of dynamic organization which is relatively independent of structural pattern and which automatically achieves adaptive functional effects. As it stands, the evidence definitely contradicts the supposition that any readaptation is achieved by spontaneous dynamic readjustment in this sense. There has been no convincing positive record of this type of recovery, whereas there are a large number of opposing accounts in which either dysfunction has persisted without any readjustment or else some adjustment has occurred but has clearly required time and practice. Functional readjustment, insofar as it has been conclusively demonstrated and especially where comparisons between man and other species are possible, appears to parallel a limited learning capacity rather than an all-pervasive plasticity intrinsic to the organization of central gray matter *per se*. It cannot be denied that some comparatively rapid adaptations may conceivably occur under particular circumstances, as after certain very favorable muscle transpositions, but the amount of central reorganization involved is relatively negligible, and the implications for the nature of central nervous organization are certainly far from the type of thing originally supposed. For practical purposes it would seem best, on the basis of the evidence now available, to relinquish altogether any hope that immediate spontaneous reorganization will effect correction of function after nerve misdirection or muscle transposition.

Local Morphological and Physiological Readaptation Phenomena

Before considering the possibilities of readjustment by the ordinary learning process, mention must be made of certain other factors essentially non-functional in character which also may contribute to readaptation, particularly after nerve regeneration. These include various growth-regulating, trophic, and other local physiological and biochemical phenomena which there has not been occasion to present in any detail in reviewing the literature. As a whole these phenomena have received little study, and little is yet known about them. Their possible significance nevertheless deserves attention. In restoration of function their effects may easily be confused with those of learning, and hence it is quite important to distinguish between recoveries due to reeducation and those due to factors such as are mentioned below.

Owing to various qualitative influences tending to prevent adverse nerve fiber terminations, to cancel the function of adverse terminations, and even to foster in a positive manner the formation and function of appropriate over inappropriate terminations, the functional results of nerve regeneration may be less maladaptive than would be anticipated from the extent of fiber shunting in the scar region. According to Elsberg (1917), for example, original motor fibers tend to succeed over foreign axons when both are given equal opportunity to reinnervate a skeletal muscle. A similar selectivity may also be exercised in the termination of the different sensory fiber types in the skin and elsewhere. According to Dale (1935), adrenergic fibers are unable to establish transmissive connections with cholinergic endings, and vice versa. Langley (1898a, 1900) reported that regenerating preganglionic fibers of the sympathetic trunk form connections discriminately in the ganglia with their own particular class of post-ganglionic neurons. There is suggestive evidence that in amphibians nerve regeneration results in a selective systematic reestablishment of original synaptic associations in the centers (Sperry, 1943c–1945). Qualitative relations between regenerating peripheral nerve fibers and end-organs may be influential in determining which of the many extra fibers that reach the periphery are to survive and attain maturity (see Young, 1942). Thus, although nerve fibers clearly can

be forced by nerve crossing to form, and do regularly form after straight nerve reunion, extensive abnormal connections, it cannot be concluded that there are no factors whatever tending to favor formation of appropriate over inappropriate terminations. Any such selectivity of regeneration and termination must depend apparently upon contact effects entirely (Weiss, 1941b).

A selective readaptation of nerve fiber connections may continue to occur after regeneration is in the main completed. For example, if the regenerated nerve terminals in the skin remain in a state of flux, degenerating and regenerating anew (see Speidel, 1942; Weddell and Glees, 1942), selective affinities favoring the perpetuation of appropriate over inappropriate relationships might gradually make the former increasingly predominant, effecting a slow sensory improvement difficult to distinguish from improvement by learning. We still do not know whether the regeneration and development of new sensory end-organs is conditioned in any way by the character of the different fiber types with which they become connected, although some approaches to that problem have been made (Dijkstra, 1933). Misdirection of salivary fibers in the region of the facial nerve leads to excessive lacrimation but not to synchronous contortions of the facial muscles. Also misdirection of the motor fibers of the facial muscles leads to mass movements but not to synchronous lacrimation and salivation. These selective effects might be due to discriminate fiber termination or to differential chronaxies. The functional effects of the regeneration of sensory fibers into motor channels, or vice versa, are commonly said to be cancelled out. Polarity of reflex conduction may be partly responsible. Incidentally, in contradiction to general belief, Ballance (1932) reported that a sensory nerve crossed to the facial could correct the muscle atrophy of facial paralysis and effectively avoid any associated movements. A predilection of outgrowing sensory and motor fibers for sensory or motor end-organs respectively has been demonstrated in development (Hamburger, 1928; A. C. Taylor, 1944). It has been suggested (Weiss, 1936, 1941c) that different skeletal motor fibers in amphibians may be selectively sensitized to respond only to specific central excitatory agents, and that this sensitization may in turn be regulated after nerve regeneration by qualitative relations in the periphery. Results obtained with sensory fibers (Weiss, 1942) have been interpreted on a similar basis. Hypersensitization produced in the nerve centers, ganglia, or periphery by denervation (Cannon and Haimovici, 1939) may result in increased function of the remaining elements, tending to correct the paralysis (see also Spiegel and Démétriades, 1925). Or the denervated parts may stimulate or permit development of collateral innervation from nearby intact fibers and thereby have their function restored (Fort, 1940; Weddell and Glees, 1942; Geohegan and Aidar, 1942; Speidel, 1942).

The foregoing is sufficient to suggest some of the many ways in which anatomical and physiological relations might conceivably be readapted locally so as to limit the abnormality of function after nerve regeneration, and in varying degrees obviate reeducation. Such factors would be of less influence after nerve crossing than after straight nerve reunion. The evidence to date has left obscure the actual extent to which readaptation is assured by such means in man and the mammals. The above heterogeneous examples are intended to be illustrative and suggestive only, and are necessarily speculative in large measure. Because of the lack as yet of positive knowledge of these aspects of recovery, one can only point summarily to the negative evidence that after all such local readaptation processes associated with nerve regeneration have been completed there still remain in mammals, following straight nerve reunion, as well as nerve crossing, extensive abnormalities and deficiencies of function which are dependent for correction upon reeducation.

The situation in the amphibians, however, is quite different, and special mention must be made of the paramount role which factors belonging in the above category play in recovery in this group. In this class of vertebrates it is a growth-regulating type of phenomenon entirely which is responsible for readaptation. Learning plays no part. After disarranging nerve connections to the limb muscles, normal muscular coordination is restored (Weiss, 1941c), and the recovery is much better than anything which occurs in the higher mammals, including man. Also after severance and various disarrangements of the intra-neural fiber pattern of the optic nerve, well organized visual perception is restored (Stone and Zaur, 1940; Sperry, 1943c–1945). Experiments have shown that these orderly and practically complete functional recoveries in the amphibians are defi-

nitely not achieved through practice and experience but are systematically predetermined by growth-regulating factors. Apparently the same factors which are responsible in ontogeny for the development of normally adaptive anatomical and physiological neuron relationships between center and periphery remain influential in adult nerve regeneration. This particular type of adaptation is correlated with the lasting embryonic lability of amphibian tissues, and apparently does not occur in the mammalian organism except in early embryonic stages (Weiss, 1935, 1936; Sperry, 1941).

After disarrangements in amphibians which cannot be entirely remedied by such growth-organizing factors and which therefore necessitate reeducative correction, as after reorientation and transplantation of muscles, limbs, and eyes, then a maladaptive effect inevitably ensues which remains uncorrected. For further discussion and analysis of the special mode of recovery in amphibians, see Weiss (1936, 1941c) and Sperry, (1943b–1945).

Recovery by Reeducation, the Learning Process

With intrinsic growth and local physiological processes of readjustment reduced to an indistinct, but what seems to be a comparatively minor, role in man and the mammals, and with spontaneous dynamic reintegration negated by the evidence, there remains what has generally been considered the major source of readaptation, the learning process itself. "Reeducation" of course implies a process of learning which is taken here to include all that is commonly encompassed by the term, from the slowest trial and error forms of learning to the rapid conceptual and insightful types. Correlated with the general lack of any precise knowledge of the neurological basis of reeducation, there has been scarcely any limit to the degree and types of central nervous reintegration which have been attributed to it. The evidence, when reviewed critically in the light of the more recent results, however, suggests that the learning capacity has marked limitations when it comes to correcting the dysfunction of misarranged nerves and end organs. Only particular forms and orders of central nervous reintegration seem to be possible, and these with a wide range of varying difficulty, while others are apparently wholly beyond the scope of the learning capacities even of man. Because of the varying effectiveness of reeducation in correcting different kinds of malfunction, it becomes of practical importance for prognosis and other purposes to try to determine in greater detail the essential neural nature of reeducation, its central locus, and its inherent limitations and potentialities.

Central locus of reeducative adjustments

There have been a number of attempts to determine the locus of reeducation in the nerve centers by cortical stimulation or by lesion methods (see Stefani, 1886; Kennedy, 1901, 1914a; Osborne and Kilvington, 1910a; Maragliano, 1912; Ballance, 1932; Bethe and Fischer, 1931; Olmsted, Margutti, and Yanagisawa, 1936; and others). In nearly all such cases, for reasons already elaborated, it is quite doubtful that the assumed readjustments had actually occurred. The results of cortical stimulation predominantly have been the same as would be expected if no central nervous readjustment had taken place. Those instances, on the other hand, in which central readjustments were indicated either by the results of cortical stimulation or by survival of adjusted behavior after decortication, cord transection, etc., appear to have been cases of spurious recovery not involving adjustment in the function of the affected parts, as supposed. There has as yet been no satisfactory experimental demonstration of the precise location in the central nervous system of the adjustments involved in reeducation.

The frequent casual references in the literature to "reeducation within the facial nucleus," "reintegration in the spinal limb centers," "readaptation of cortico-spinal connections," and the more emphatic assertions of Anokhin, Goldstein, Bethe, Osborne and Kilvington, and the rest that even the most basic phylogenetic reflex associations of the spinal system undergo adaptive reintegration after nerve-muscle rearrangements are all without reliable experimental foundation. The evidence on the whole tends to refute such assumptions and to indicate that reeducative neural adjustments, like those of learning under normal conditions, are confined to higher brain relations and never effect any switching of basic neuron associations of the spinal cord or of the primary sensory and motor nuclei of the brain.

The question may be raised, however, whether the attainment of automaticity by long practice might not result eventually in descent of the central reorganization to lower motor levels. Reliable direct evidence is lacking on this point, but

what little is known of the basis of habit automatization in general (Lashley, 1921) suggests that the descent of central reorganization to subcortical levels, if it occurs at all, is not sufficient to make an acquired habit independent of its cortical organization. Weiss and Brown (1941) reported relapses into old motor patterns many years after transplantation of the hamstring muscles in man, indicating that the original basic coordination patterns remain intact under superimposed learned adjustments (see also Weiss and Ruch, 1936). It remains very doubtful that even continued practice effects any remodeling of the basic spinal and brain stem integration patterns.

Neural nature and limitations of reeducation

What little is known of, and further, all that is known *not* to be true of the neural basis of learning under normal conditions (Hilgard and Marquis, 1940) seems to apply to reeducation after peripheral disarrangements. There is no indication that reeducative neural adjustments following nerve regeneration and muscle transposition are any different from those involved in learning under normal circumstances. They would seem not to be any more elementary or simple, nor their location in the brain any more circumscribed than are the still obscure and elusive engrams of ordinary habits of motor skill and sensory discrimination (Lashley, 1931).

The commonly recognized laws and limitations of learning such as pertain, for example, to the transfer of training from one performance to another, to the complexity of performance, frequency of repetition, speed, etc. apply, as far as can be ascertained, equally well to reeducation after nerve-end organ rearrangements. Further discussion, however, is restricted to mention only of certain limitations of the learning process particularly pertinent to the conditions of nerve regeneration and the transposition of muscles and sense organs. It is highly significant from the theoretical viewpoint that the results of experiments on nerve interchange and end organ transposition suggest characteristic and basic limitations of the learning process which were not previously brought out by observations under other conditions, and which may serve as important clues to the essential character of the neural basis of the learning process itself.

On the motor side, the evidence suggests a limit in the minimum size of the functional units which are subject to dissociation and recombination by learning. Corollary to the conclusion that learning depends upon reorganization in the higher centers is the further deduction that reeducative dissociation and reintegration of motor patterns must therefore be restricted by the degree of refinement in the relations between the higher centers and the spinal motor system. One would consequently expect to see an increase in capacity for effecting detailed motor reorganization by reeducation in passing from the lower vertebrates through the mammals to man, correlated with the appearance and increasing elaboration of the cortico-spinal system. A marked increase of this kind is indicated by the data available. The urodele amphibian, for example, does not correct or even inhibit the normal action of the entire limb after transplantation has made the normal limb coordination detrimental to the animal (Weiss, 1937b). In the lower vertebrates it appears to be only generalized movements and orientations of the whole organism that can be recombined by learning. Even the rat is apparently not able to correct or even inhibit separately the movements of one limb joint after muscle and nerve crosses, indicating that the minimum functional units subject to dissociation and recombination by learning are considerably larger in the rat than in man and involve possibly movements of the limb as a whole. In man, not only the action of a single limb joint but even that of individual muscles about a joint can be dissociated and readapted to some extent. The functions of individual motor neurons, however, are not dissociated and reorganized into new adaptive groupings even in man. The minimum functional unit subject to recombination by learning in man seems, therefore, to be approximately the "motor pool" of individual muscles or of the gross subdivisions of muscles with multiple heads. Such minimum recombinable motor elements may vary, however, under different conditions not only between species but also between different parts of the body, exhibiting, for example, greater refinement in the arm than in the leg.

It is important, in view of the foregoing, to distinguish the different orders and degrees of motor readjustment that may be achieved in any single task of readaptation. For example, after nerves have been crossed experimentally to antagonistic muscle groups of the dog's hind limb, varying degrees of improvement over the reversed movements which would otherwise follow, might be effected (1) by simply inhibiting all movement

of the affected leg; or (2) by inhibiting movement only of the particular joint involved; or (3) by contracting all the effected muscles as group indiscriminately, to provide a constant rigidity to the joint; or (4) by contracting persistently one of the antagonistic groups alone, to keep the joint fixed in a definite favorable position; or (5) by reversing the timing of one of the antagonistic groups alone, to provide stability of the joint selectively at only the proper phase of leg movement; or (6) by reversing the timing of both groups, to achieve approximately a recovery of correct coordination; or (7), since optimal function would not result with all muscles of each nerve acting as a unit in perfect synchrony, further improvement would be achieved by more refined differentiation and readjustment in the timing and intensity of the individual motor neurons. The central nervous reorganizations involved in these different possible methods of adjustment may vary greatly in their essential nature and in difficulty of achievement. The last and most refined order of reintegration at the neuron level is not required after transposition of muscles, making the task of complete reeducation much easier after muscle transposition than after nerve regeneration. For further discussion of the limitations of reorganization in connection with the functional individuation and hierarchical organization of the nervous system, see Weiss (1941c). Additional limitations and difficulties of motor reorganization correlated with some other types of factors have also been presented above in the section on muscle transposition.

On the sensory side, the possibilities of reeducation vary greatly, according to the nature of the central and motor effects which are produced by the afferent stimuli concerned. Afferent excitations are in part projected to the cerebral cortex but are also short circuited through spinal, cerebellar, thalamic, and other lower levels and may result in any one or all of a variety of different effects, such as the following: (a) immediate overt reflex responses initiated involuntarily before the subject is aware of any sensation; (b) modification and regulation of motor activity without the subject's awareness, as with proprioceptive stimuli; (c) sensations of which the subject is aware and as a consequence of which motor activity is deliberately initiated or altered; (d) sensations which register but which lead to no direct overt response. This list is not intended to be comprehensive (see also Weiss, 1941a) but merely sufficient to serve as a reminder that the central functional associations of different afferent fibers, and even of single fibers, are complex and varied. The problems and possibilities of readapting these central associations are correspondingly varied. In so far as the sensory derangement affects neutral stimuli which lead to no overt reaction, almost any adaptive response could presumably be "conditioned" to the neutral stimuli. On the other extreme, where the stimuli involved evoke spinal reflexes, there arises the problem of readaptation at the spinal level. It is very doubtful that the integrative effects of afferent impulses (Creed, 1931) at the spinal level, or even at cerebellar or other sub-cortical levels, can be directly readjusted in the higher mammals by reeducation. Any improvement must probably come, therefore, in an indirect manner via superimposed activity involving cortical organization. It is still not certain that the central sensory excitation patterns of immediate subjective sensation are subject even to relatively simple corrections in man by any amount of reeducation. Adaptive adjustment of overt voluntary reactions to compensate for certain simple derangements of sensation, on the other hand, has been shown to be not difficult in man, and possible even in the rat.

The task of recovering refined discriminatory sensory functions, following the random regeneration of sensory fibers with consequent disruption of the orderly sensory projection to the cortex, raises special problems which have received little clarification thus far. That errors in mode of sensation owing to the termination of fibers on the wrong class of end organ can ever be corrected by reeducation has been considered unlikely, but there is no actual evidence on the matter. Readaptation of disturbed localization, however, has commonly been assumed to occur. It is quite conceivable that with practice one might learn to localize correctly, under the conditions of random reinnervation, one or a number of individual cutaneous points whose localization was at first misreferred. It seems very doubtful, however, that any amount of practice could restore normal stereognosis and other discriminatory performances involving areal pattern stimulation and necessitating proper adjustment with respect to the relative spatial relations of large patterns of myriad end-organs supplied fortuitously by misdirected fibers. Even the most basic spatial arrangement of cutaneous innervation permitting determination, for example, of whether one point

or two is being stimulated, the relative location of two points or of a series of points with respect to each other and the analogous basic organization of proprioceptive innervation upon which pattern perception depends will be completely distorted under such conditions. The task of relearning the basic spatial and proprioceptive values of all the individual misdirected fibers and of all their interrelationships after disarrangement at the neuron level would be one of tremendous complexity. Under normal conditions these basic sensory values are provided automatically through an innate orderliness of associations between the central sensorium and periphery, an innate orderliness which presumably is established in ontogeny by growth processes and not by learning. This interpretation is contradictory to the conclusion of Douglas and Lanier (1934) that sensory "local signs" depend mainly on habit formation; but it is supported by the observations of Purdy (1934), Ewert (1936), Sperry, (1942b–1945), and by other lines of evidence (see especially von Senden, 1932; Hebb, 1937).

Stopford's (1930) contention that "epicritic" cortical sensations are separable from and more subject to reeducation than "protopathic" thalamic sensations is, of course, speculative and based originally upon erroneous interpretations of sensory recovery that have since been abandoned (Walshe, 1942); but too little is yet known to make any final negation or affirmation of the general idea itself. This, along with most of the problems of sensory recovery, only some of which have here been indicated, remains more properly a matter for future investigation than for present speculation or review.

Other things being equal, the degree of differential voluntary control over the functions of the autonomic system tends to be less than over somatic functions, and the evidence indicates a correspondingly greater refractoriness of the autonomic functions to dissociation and reorganization.

It has occasionally been implied or directly suggested, in regard to the substitution or crossing of nerves, that probably (Harris and Low, 1903) the nearer the roots of the two nerves are to one another in respect of the cord segments to which they belong, or (Frazier, 1924) the nearer together the cortical centers presiding over the movements concerned, the easier will be reeducative correction of any incoordination. The choice of nerves for crossing as well as the choice of muscles for transposition would seem in this regard to be primarily a matter of functions subserved, rather than of topographical proximities of nerve roots or motor centers. This is consistent with the conclusion that the reeducative adjustments are not effected in the primary motor or sensory nuclei but involve the higher association centers, and that it is not merely a simple localized switching of anatomical connections which occurs but an operationally organized readjustment of the sort involved in ordinary learning under normal conditions.

ACKNOWLEDGMENT

This review was prepared as part of a project directed by Dr. Paul Weiss under contract, recommended by the Committee on Medical Research, between the Office of Scientific Research and Development and The University of Chicago.

I am deeply grateful to Dr. Weiss for encouragement in the work and for his painstaking constructive criticism of the manuscript.

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NEW BIOLOGICAL BOOKS

The aim of this department is to give the reader brief indications of the character, the content, and the value of new books in the various fields of Biology. In addition there will frequently appear one longer critical review of a book of special significance. Authors and publishers of biological books should bear in mind that THE QUARTERLY REVIEW OF BIOLOGY *can notice in this department only such books as come to the office of the editor. The absence of a book, therefore, from the following and subsequent lists only means that we have not received it. All material for notice in this department should be addressed to B. H. Willier, Editor of* THE QUARTERLY REVIEW OF BIOLOGY, *Department of Biology, Homewood Campus, The Johns Hopkins University, Baltimore 18, Maryland, U. S. A.*

WHAT IS LIFE? AND WHAT IS TRUTH?

A review of *What Is Life? The Physical Aspect of the Living Cell.* Based on a course of public lectures delivered under the auspices of the Institute for Advanced Studies at Trinity College, Dublin, Ireland, in February, 1943. By *Erwin Schrödinger. Cambridge, England* (Cambridge University Press) and *New York* (The Macmillan Company). $1.75. viii + 91 pages; plates. 1945.

By Max Delbrück, Department of Physics, Vanderbilt University.

The author of this little book proposes to discuss the large and important question: "How can the events *in space and time* which take place within the spatial boundary of the living organism be accounted for by physics and chemistry."

This is a broad question, embracing the problem content of many branches of the biological sciences. Biologists with a romantic reverence for the powers of modern physics might be led to expect great revelations from the book. It turns out, however, that the author's aims are much more modest. It is not at all in the "how" of the accounting that the author is interested but in the question *whether* physics and chemistry will be able to give a *complete* account. This is not so clear-cut a question as might appear, since the terms physics and chemistry are ill-defined when applied not to the present but to the future content of these sciences. It is also a question whose answer involves prophecy, and there is not much point in arguing about prophecy. In his main discussion the author therefore climbs down to a still more modest ground and defines as his aim the establishment of this point of view:

"The obvious inability of present-day physics and chemistry to account for such events is no reason at all for doubting that they can be accounted for by those sciences."

The argument runs as follows:

In physics we have learned to describe the well determined behavior of systems by two types of law. Either the systems are made up of large solid bodies like clocks, and follow dynamical laws unperturbed by molecular heat motion; or they are made up of very large numbers of statistically independent elements, and observation concerns the average behavior of these elements. We are then dealing with statistical laws that appear to be accurate laws because the numbers of particles involved are so large that statistical fluctuations are negligibly small. The diffusion law and the mass action law are examples of this type of law in which "order comes from disorder." In either of these two types of well determined behavior, it is essential that individual molecules should have no measurable effect on observable quantities.

Being well acquainted with this situation, the naive physicist would expect that the living cell, exhibiting a well determined behavior, is constructed on the same principles, i.e., that it functions like a clock or like a thermal machine, or some mixture of the two. In any event, he would expect it to be well protected from being thrown off balance by statistical fluctuations. Therefore, if the cell contains molecular species with an important rôle in its makeup, he would expect each such molecular species to be represented by a very large number of individual molecules. In these expectations the naive physicist finds himself flatly contradicted by the facts of life. Far from being a mass action system, either in the dynamic or in the thermodynamic sense, the living cell is controlled by genes which are present in single or double copy, and which are handed on from cell to cell by an unknown mechanism of reproduction.

This outline of the naive physicist's dilemma is followed by an exposition of some of the elements of genetics, of the results of radiation genetics, and of other data bearing on the mechanism of mutation

and on the stability of the genes. The discussion leads up to the concept of the gene as an "aperiodic crystal." The genes are given this startling name rather than the current name " complicated molecule," because the author wishes to emphasize, first, that the stability of the genes is of the same kind as the stability of crystals in which the constituent atoms are held together by valency bonds, and second, that these crystals are of a new kind, not previously investigated by physicists who concentrated their attention on periodic crystals. There is nothing new in this exposition, to which the larger part of the book is devoted, and biological readers will be inclined to skip it.

Schrödinger lays great stress on the fact that the stability of the genes is a quantum mechanical stability. We know that the stability of atoms and molecules is deeply rooted in quantum mechanics, and our general understanding of molecular stability has been of help when the stability of genes was investigated. This indebtedness to quantum mechanics is not a private debt of biology. It is part of the general debt of chemistry to quantum mechanics. In Schrödinger's opinion, the dependence of the well determined behavior of living organisms on this new and little studied physical structure, the "aperiodic crystal," explains the obvious inability of present day physics and chemistry to account for this behavior. In his opinion the inability is similar to the inability of a man who knows how a steam engine works and who is confronted with an electric generator. Both of these contraptions contain iron and copper, but unless he has studied electrical phenomena the steam engineer will be quite unable to understand the working of the generator. He might be tempted to believe that the laws of physics break down when applied to the generator, while in reality he is merely confronted with the behavior of matter under a new set of conditions which he has not yet analyzed.

This bold analogy will seem very suggestive to many readers. But is it a valid analogy? One must note that it is based in large measure on a substitution of terms. For the term "complicated molecule" he substitutes the term "aperiodic crystal," the latter term implying a new and unexplored state of matter.

After this profession of faith in the physical nature of the workings of the cell, Schrödinger attempts a further characterization of its mechanism, as one which produces order from order, in contradistinction to the statistical mechanism where the orderliness of the observed phenomena is the result of the disorder of the molecules. The clockwork is also based on the order from order principle. The basic similarity between the clock and the living cell is seen in the fact that both depend on the solidity of the controlling parts, the clockwork on massive, man-made solids, the cell on the aperiodic crystalline genes. The essential difference between the clock and the living cell is seen in the fact that in the clock the disordering tendencies are kept at bay by operating with solids of large mass, while in the living cell the decay to thermodynamic equilibrium is evaded by continually drawing on sources of negative entropy from its environment.

While it is of course correct to say that life processes depend on the availability of negative entropy, such a statement is at best a partial description of the tricks of life. It fits any system of enzymes acting on their substrates. Translated into the usual terminology it reads: the living cell employs specific enzymes, and these enzymes promote selectively some of the reactions which involve an increase in free energy.

The author does not return in his later discussion to the problem of how the cell gets around the statistical fluctuations. The careful reader will be disappointed by this omission. At the beginning of the book the statistical fluctuations are represented as an unsurmountable obstacle to the physical understanding of the cell, but later on this difficulty seems forgotten. Without a finer discussion of this aspect, particularly for enzymatic processes and for non-steady states, the argument of the book loses its strength. How does the cell manage to produce just one replica of each gene at each cell division? Are we not here dealing with small numbers of molecules, and should we not expect large fluctuations? Perhaps, as some believe, more than one replica is formed, and the supernumerary ones are shed into the cytoplasm. But why do these extra pieces not cause havoc in the cytoplasm? How are the fluctuations in the numbers of these extra pieces brought under control? Perhaps our present knowledge of cellular processes is insufficient to make such a discussion convincing. If that be the case, then any statement about the physical nature of cellular organization would appear premature.

Schrödinger delimits his stand further by stating that, in his opinion, "and contrary to the opinion upheld in some quarters, *quantum indeterminacy* plays no biologically relevant role" (in the cell). The opinions here referred to are presumably those of Bohr and those of Jordan. This is not the place to take up the challenge for these authors, but it is to be hoped that they will themselves continue the discussion.

There is a brief epilogue on "Determinism and Free Will." Having declared his belief that the events in the cell, if not strictly deterministic, are at least statistico-deterministic, the author attempts in this epilogue to bring this belief into harmony with the "immediate experience" of a free will, i.e., with the experience of directing the motions of one's body, of which motions one foresees the effects. How can the conscious "I" direct something whose course is set by the laws of nature? Schrödinger thinks that the only solution to this dilemma lies in the assumption that the "I" and the laws of nature are one and the same thing. It must then be assumed that the "I" of immediate experience is a singular of which the

plural is unknown. He draws on the scholars of the Vedanta and on other sources to support the view that the plurality of consciousness is an illusion. The reviewer finds himself unable to define his reaction to this epilogue.

Undoubtedly the book will attract many readers, because of its title, because of the very high respect which we owe to its author, and because of its clear, simple, and forceful style. It will have an inspiring influence by acting as a focus of attention for both physicists and biologists. Physicists will be interested in the book because it will to some extent answer their curiosity about biology. Biologists will find it hard to appreciate the dilemma of the "naive physicist" outlined at the beginning of the book. Physicists and biologists who are not familiar with Bohr's subtle complementarity argument will be inclined to take the physical nature of the cellular processes for granted at the outset, and may be dissatisfied because Schrödinger does not advance our understanding of cellular mechanisms in any specific respect. The reviewer believes, however, that Schrödinger's discussion of the types of laws of nature might exert a clarifying influence on biological thinking.

REVIEWS AND BRIEF NOTICES

GENERAL BIOLOGY: PHILOSOPHY AND EDUCATION

THE PHILOSOPHY OF BERTRAND RUSSELL. *The Library of Living Philosophers, Volume V. Edited by Paul Arthur Schilpp. Northwestern University, Evanston and Chicago.* $4.00. xv + 815 pp. 1944.

In each volume of this "Library" some twenty exponents and opponents of the thought of an outstanding philosopher contribute expository or critical articles, and the philosopher himself submits a reply. There is also included an intellectual biography (or autobiography) of the thinker under discussion, along with a complete bibliography of his works. One very welcome result of such a project is to decrease the differences of opinion which inevitably arise as to what some of the profoundest writers of our age really intended to say.

The present volume is of special interest to scientists, because Russell came into philosophy from mathematics and has contributed to our understanding of the mathematical and other scientific methods of inquiry. Russell has tried to introduce "scientific method" into philosophy and has been a champion of the "empirical approach" to all but a few of the most abstract problems. He has also helped to work out the philosophical implications of some of the more striking scientific discoveries as well as the social implications of the scientific movement as a whole. Many of the authors pay tribute to other contributions of Russell to our age, notably, his inventions in the way of exact techniques for the attainment of the clarity and simplicity of thought for which he has such a passion, and his efforts in the interests of freedom, democracy, and social harmony.

Russell's critics very naturally call attention to the incompletenesses, confusions, and contradictions in his work and to the occasional failure of his theories to explain pertinent facts. Over-simplification is said to be inherent in his analytic and reductive tendencies, his ways of defining, his choice of indefinables, his search for a "minimum vocabulary," and his very assiduous use of "Occam's razor." He is also taken to task for creating "dualisms," for an occasional wavering between two irreconcilable trends of thought, and for an unreasonable amount of skepticism and pessimism. Some will think that the most serious charges levelled at him are: that his approach to social problems is "abstract and unhistorical"; that he misunderstands religion; and that his "ethical subjectivism" leaves him powerless before the onslaughts of a philosophy of power.

Russell very frankly acknowledges the justice of a few of these criticisms; and one feels that he successfully meets many of the remainder—either by new buttressings and extensions of his position or by showing that the charges arise from misinterpretations of what he has already said.

Russell's account of his intellectual development and his "Reply" afford many fascinating side-lights on his present stand, only a few of which can be mentioned here. Early in his career Russell deduced mathematics from logic; but now he believes that logic itself depends on psychology and physiology. A scientific account of reality, he holds, must employ both induction and deduction; but neither of these can be deduced from the other, and the principle of neither can at present be "proved." "The canons of scientific inference have never yet been formulated," he adds; "whether. . .an individual decides to accept or reject these canons, is a purely personal affair, not susceptible to argument. . ."

According to Russell, reality can be analysed into three kinds of "events": "unperceived events," sensations, and images. He contends that things, matter, electrons, waves, the self, etc., are merely series (or groups) of "events". In addition to "particulars," he thinks, at least one universal is essential to an adequate description of reality. The physical world, he holds, is best regarded as a four-dimensional manifold of "events"; and our knowledge of the physical world is merely a knowledge of "structure." Causal laws, he adds, may be assumed to be the same for living matter as for dead matter.

Russell believes that, although scientific laws are discoverable in the economic and statistical portions of history, there are determinants of the past other than the economic, so that we cannot expect to make history completely scientific. The knowledge that it gives of human beings, he maintains, is "not mainly analytic scientific knowledge, but the sort of knowledge a dog-lover has of his dog."

Science is ethically neutral, says Russell; it is a tool which cannot determine the purposes for which it should be used. It follows that science contains no basis for a belief in progress; and, since scientific method is the only rational method, that ethics has no rational basis. The good is simply the desired, he thinks; and a value judgment is merely an expression of a desire for something, together with a wish that all other persons have the same desire. Russell concedes that his social, political, and educational beliefs have no logical connection with his logic and epistemology; and he admits that these beliefs are just so much propaganda for democracy, freedom, and cooperation—which he personally happens to desire. It is at this point that he becomes defenseless, logically, against a philosophy of power.

To those who remember Russell's attitude toward religion in *Free Thought and Official Propaganda*, his announcement in this book that although religion does not represent truth, nevertheless "some form of personal religion is highly desirable," will come as something of a surprise. Similarly, his agreement with Einstein that "fear of metaphysics is the contemporary malady" may seem odd in view of his devotion to scientific method and his passion for simplicity.

As to trends, some of the expositors of Russell's thought find that it moves in the direction of empiricism and nominalism, while Russell himself remarks that in ontology he is becoming increasingly materialistic, and in theory of knowledge, increasingly subjectivistic.

It is of course foolish to expect agreement on a choice of contributors to a volume such as this; nevertheless, the omission of essays by such outstanding students of Russell's thought as John Dewey and Arthur O. Lovejoy—to mention only two of those who are conspicuous for their absence—will be disappointing to many. However, the volume is rewarding in a variety of ways and deserves a wide audience.

RAYMOND P. HAWES

PRINCIPLES OF MODERN BIOLOGY. *A Complete Reconstruction and Modernization of Charles R. Plunkett's Elements of Modern Biology.*

By Douglas Marsland. Henry Holt and Company, New York. $3.75. ix + 774 pp. 1945.

Being one of those who considered Plunkett's "Outline of Modern Biology" an outstanding textbook, the reviewer opened Marsland's revision of the 1930 edition with a mixture of hope and anxiety. Much of the hope and a little of the anxiety have been fulfilled.

The book has been completely reorganized. Instead of the first four parts, which consisted of chapters dealing with protoplasm, nutrition, response, and reproduction, there are now only three—on the cell, multicellular plants, and multicellular animals (with special emphasis on man). To the reviewer, this seems to be a more practical arrangement than the old for teaching, and the change is welcome. Equally welcome are the expansion of the single chapter on nutrition in multicellular animals (formerly of about 50 pages) into several chapters (now over 100 pages), dealing separately with digestion, metabolism, circulation, respiration, and excretion; and the inclusion of new material on such subjects as plant movements, viruses, hormones, and vitamins.

Unfortunately, there have been cuts as well as additions. One sad loss is incurred by the failure to include anything from the early edition on the determination of symmetry in development, another by omitting the section on the geographic distribution of organisms from the chapter on "The Results of Evolution"—a section which might well have been enlarged rather than eliminated. A number of photographs have been added to the text, and would doubtless enhance it had they not suffered from very poor reproduction—so poor that some of them are at present worthless. It is to be hoped that this will be corrected in later printings.

On the whole, though, it seems that the revision has brought Plunkett up to date fairly satisfactorily without sacrificing the stimulating quality of the earlier text, and the new Marsland and Plunkett deserves a trial.

EILEEN SUTTON GERSH

THE BOOK OF FROM SEED TO TREE.

THE BOOK OF QUEER ANIMALS. *Nature Science Books Outdoor Adventures.*

By Charles H. Gable and Ellen Schulz Quillin. Albert Whitman & Company, Chicago. 50 cents each. 48 pp.; 47 pp. 1944.

These stories about nature study are written for children of six to ten years, by a Texas public school teacher of nature study and science and the director of a museum in that state. Children will like them. There is an interesting attempt to encourage and illustrate scientific thinking. The account of photosynthesis in the first book listed is somewhat incorrect. The pictures appear crude in comparison with those in some other children's series.

ECOLOGY

CHEMICAL RESULTS OF THE LAST CRUISE OF THE CARNEGIE. *Scientific Results of Cruise VII of the Carnegie during 1928-1929 under Command of Captain J. P. Ault. Chemistry—I. Department of Terrestrial Magnetism, Carnegie Institution of Washington Publication 562.*

By Herbert W. Graham and Erik G. Moberg. Carnegie Institution of Washington, Washington. $1.25 (cloth); $1.00 (paper). vii + 58 pp. 1944.

This report considers the distribution in the sea of phosphate, silicate, hydrogen ions, and oxygen. The findings will be of considerable interest to ecologists and students of plant and animal distribution. The striking excess of phosphate and silicate in the Pacific as compared with the Atlantic, the deficiency of O_2 in the deep waters of the Pacific, and the strong negative correlation of phosphate concentration and pH are surely not without their important biological corollaries.

OBSERVATION AND RESULTS IN PHYSICAL OCEANOGRAPHY. *Scientific Results of Cruise VII of the Carnegie during 1928-1929 under Command of Captain J. P. Ault. Oceanography—I-A. Department of Terrestrial Magnetism, Carnegie Institution of Washington Publication 545.*

By H. U. Sverdrup, J. A. Fleming, F. M. Soule, C. C. Ennis. Carnegie Institution of Washington, Washington. $2.50 (cloth); $1.75 (paper). v + 156 pp. 1944.

This report deals mainly with determination of ocean depths, subsurface temperatures, densities, salinity, and the collection and preservation of bottom samples.

I. MARINE BOTTOM SAMPLES COLLECTED IN THE PACIFIC OCEAN BY THE CARNEGIE ON ITS SEVENTH CRUISE, *By Roger R. Revelle.* II. RADIUM CONTENT OF OCEAN-BOTTOM SEDIMENTS, *by Charles S. Piggot. Scientific Results of Cruise VII of the Carnegie during 1928-1929 under Command of Captain J. P. Ault. Oceanography—II. Department of Terrestrial Magnetism, Carnegie Institution of Washington Publication 556.*

Carnegie Institution of Washington, Washington. $2.50 (cloth); $2.00 (paper). ix + 196 pp. 1944.

The description of marine bottom samples in this report has value for marine ecologists interested in the distribution of calcium carbonate in the Atlantic and Pacific Oceans, or of globigerina, radiolarian, and diatom oozes and muds. Other organic constituents of the samples include traces or minor amounts of sponge spicules, echinoid spines, and ostracod shells, and rare remains of bryozoan, molluscan, and vertebrate forms.

THE GENUS CERATIUM IN THE PACIFIC AND NORTH ATLANTIC OCEANS. *Scientific Results of Cruise VII of the Carnegie during 1928–1929 under Command of Captain J. P. Ault. Biology—V. Department of Terrestrial Magnetism, Carnegie Institution of Washington Publication 565.*

By Herbert W. Graham and Natalia Bronikovsky. Carnegie Institution of Washington, Washington. $2.50 (cloth); $2.00 (paper). vii + 209 pp. 1944.

Fifty-eight species of *Ceratium*, including three new species, one new subspecies, and three new varieties, were collected by the "Carnegie" in the North Atlantic and Pacific Oceans. Their horizontal distribution seems to be unaffected by salinity or absolute phosphate content. Temperature is a more significant factor, the species (and subspecies) being divided into five groups: intolerant tropical (23); slightly tolerant tropical (23); very tolerant tropical (5); cosmopolitan (5); and subpolar (5). Currents are also certainly important. It is interesting that the eight rare solely Pacific species are either intolerant or slightly tolerant tropical species. It is noteworthy that tolerance of individual species may be decidedly different in the two oceans. The vertical distribution of *Ceratium* species was found, except for three species, to agree with Nielsen's theory of the deeper distribution of "shade forms" marked by thin, often expanded bodies crowded with chromatophores. The observed vertical migrations of Ceratia cannot be accounted for wholly by phototropism. The phototropism may itself depend on the supply and assimilation of certain nutrients. The figures and charts add to the value of this significant contribution to the study of plankton distribution.

EVOLUTION

LAMARCK, *Interprete de la Naturaleza.*

By Enrique Beltran. Sociedad Mexicana de Historia Natural, Mexico, D. F. (Paper). xv + 161 pp. 1945.

This essay commemorating the 200th Anniversary of the birth of Lamarck, by a distinguished Mexican biologist who is professedly an admirer of the 'Founder of Evolution,' covers familiar ground for the most part. However, by removing Lamarck, the man, from the arena of subsequent struggles involving Lamarckism and neo-Lamarckism, a juster picture of the great evolutionist appears. The work of Lamarck is considered in relation to his epoch, the stormy years of the French Revolution with which he identified himself,

to his forerunners, and to his contemporaries, the giants Buffon and Cuvier, whom, in historical perspective, the humble guardian of the king's herbarium was to eclipse. The author, who cleaves to neither of the Lamarckian schools in the controversial sense usually understood in biology, brings to date (1944) the status of 'lamarckismo,' that formidable rock in the evolutionary stream.

Excerpts from the works of Lamarck—'hidden treasures'—translated into Spanish, form the second half of this essay, to which a bibliography of Lamarck is appended.

H. R. CATCHPOLE

EXPERIMENTAL STUDIES ON THE NATURE OF SPECIES. *II. Plant Evolution through Amphiploidy and Autoploidy, with Examples from the Madiinae. Carnegie Institution of Washington Publication 564.*

By Jens Clausen, David D. Keck, and William M. Hiesey. Carnegie Institution of Washington, Washington. $2.00 (cloth); $1.25 (paper). vii + 174 pp.; 4 plates. 1945.

This represents another invaluable contribution to the modern study of evolution. The first part of the study reports fully the results of investigations of three amphiploids, *Madia nutramii*, *M. citrigracilis*, and *Layia pentaglossa*. The first of these was artificially synthesized by a doubling of the chromosome number in the sterile hybrid between two geographically isolated species of *Madia*, differing in some fifty morphological characters as well as in chromosome number. Full fertility was attained by the amphiploid in the fourth generation. In the garden at Stanford, it exceeds both parents in vigor, and must be regarded as a good species, since it differs from all other species by characters of specific magnitude in the genus, reproduces only its own kind, and "is genetically isolated from all other species."

M. citrigracilis occurs as a natural species in a region where the distributions of *M. gracilis* and *M. citriodora* overlap. It has been resynthesized through hybridization of these two species, followed by a doubling of chromosome number in the highly infertile hybrid. *L. pentaglossa* is an artificial amphiploid similarly derived from *L. pentachaeta* and *L. platyglossa*. In one spot where the distributions of the parent species overlap, two similar natural amphiploid plants were found.

The second section of the book considers the place of amphiploidy in biosystematic relationships. The biosystematic units are defined as follows: The *ecotype* is a genetically and physiologically distinct ecologic race, there being no genetic obstacle to a free interchange of genes between ecotypes of the same species. The *ecospecies* manifests genetic barriers to the free interchange of genes with other ecospecies of the same cenospecies. The *cenospecies* is completely unable to exchange genes with other cenospecies of the same comparium, but by amphiploidy the genes of distinct cenospecies may be combined in a new species. The *comparium* is completely isolated from other comparia even by way of amphiploidy. The evolutionary processes are related to these levels in fundamental ways. Thus evolutionary processes initiated solely by hybridization, such as amphiploidy, chromosome exchange, or gene interchange, "have their best opportunities in comparia consisting of many cenospecies, with many ecospecies and ecotypes." The requirements for a successful amphiploid are outlined.

Experimental polyploids are then classified on these principles, in the most complete and well organized review available. Seventeen clear cases of amphiploids are grouped as either inter-ecospecific or inter-cenospecific, and it is made clear that the great majority of successful ones fall into the latter class, with parental genomes that show no inter-pairing and preserve their identity. Other examples of amphiploidy, in *Lactuca*, *Prunus*, and especially in *Rubus*, are reviewed, making 31 cases of ploidy in all.

In the next chapter the ecologic characteristics of eleven natural amphiploids are considered. It is found that in general they occur in environments intermediate between those occupied by the parent species, and in nearly half the cases appear to grow also in the habitats of their parents and become very successful. This chapter is followed by a similar consideration of ten natural autoploids. Simple doubling of all chromosomes in a plant may clearly enable the new form to occupy a new ecologic niche in some cases, and thereafter lead through selection to distinct diploid and tetraploid species. Changes from annual to perennial habit may result. Amphiploidy may be superimposed upon autoploidy.

The final chapter concludes with a summary of the authors' ideas regarding evolutionary sequence. The stages in this sequence are the ecotypes, ecospecies, cenospecies, and comparia. Amphiploidy has a rejuvenating effect, so that cenospecies may be reunited. "Youthful" groups have an abundance of ecotypes within a few ecospecies. A monotypic cenospecies in a single comparium is genetically "senescent." After the possibilities of amphiploidy and autoploidy are exhausted, apomixis may ensue. That is the final flare-up of variability in the group. Beyond this waits only extinction.

Every student of evolutionary problems will want to give this notable publication his deepest and most serious study.

BENTLEY GLASS

THE MORAL THEORY OF EVOLUTIONARY NATURALISM. *By William F. Quillian, Jr. Yale University Press,*

New Haven; Humphrey Milford, Oxford University Press, London. $3.00. xiii + 154 pp. 1945.

It is quite natural that a book on the moral theory of evolutionary naturalism should begin by informing the reader what naturalism is, or at least what it is construed to be, as interpreted by the author. One is prone to think of the term as an antonym to supernaturalism. This implication is apparently accepted by the author, who refers to it as "that metaphysical view which holds that 'the universe requires no supernatural cause or government, but it is self-existent, self-explanatory, self-operating, and self-directing." It scarcely needs to be pointed out that there are many individuals eminent in the fields of science and philosophy who hold that all events in the universe transpire in accordance with natural laws and who would therefore consider themselves adherents of the doctrine of naturalism, but who entertain all sorts of views as to the metaphysical and the theological implications of this standpoint. With so many varieties of naturalism, opponents of this doctrine enjoy a considerable latitude in construing the term to suit themselves, and they have little difficulty in supporting their interpretations by quotations from authorities of high standing. After citing a statement by T. H. Huxley, Quillian tells us: "Thus, naturalism is seen to be that philosophical view which claims that the only knowledge we can have is of the world which is known through sense perception and which is the subject-matter of the natural sciences." Perhaps the author has overlooked Huxley's own emphatic rejection in his essay on Science and Morals of a very similar interpretation of his position made by Lilly. A naturalism which denies the possibility of knowledge of mathematics, of the psychology of reasoning, and of the moral emotions, because they are not known through sense perception, is a rather preposterous man of straw. But despite the author's unfortunate definition, his later discussions are not hampered by a strict adherence to it.

The second chapter, on "The Moral Theory of Evolutionary Naturalism," is chiefly expository of the views of Darwin, Spencer, Clifford, Leslie Stephen, Westermarck, and Guyau. In the following chapter, devoted to a criticism of this theory, the chief topics considered are the theories of the origin of the moral sense and the moral standards that have been derived from the fact of evolution. The arguments follow the usual line found in most of the many books and articles opposing a naturalistic ethics. It is conceded that "natural selection. . .has played an important part in the origin and development of primitive morality," but it is contended that "this naturalistic account. . .fails completely to explain the unique characteristics of the moral consciousness." The evolutionary naturalists are said to have fallen into the "genetic fallacy" which is based on "the belief that an adequate explanation of a phenomenon is rendered by tracing it to its origin." Even if the evolutionists can account for the way in which our moral sentiments have been evolved, "evolutionary ethics," we are told, "has fallen down just at the point where it tries to explain the nature and meaning of normative morality," which is concerned with "that which ought-to-be as distinguished from that which is." And normative morality is said to have "metaphysical foundations" which "have no place in a naturalistic theory of reality or of ethics."

The evolutionary moralists are accused of ignoring or explaining away the really essential features of the moral life. "If this 'nothing-but' point of view were true," Quillian asks, "what would be left of the moral life as we know it? There would be no moral judgments and no choice, for there would be no *understanding* of one's self or of other selves and of their relationships. . . .There would be no consciousness of obligation, no awareness of what I ought to be but am not."

Many writers on ethics seem to derive a peculiar satisfaction in portraying the dire consequences that would follow from consistent adherence to a purely naturalistic morality. Quillian's volume carries out this tradition in its usual form. The godless evolutionary naturalists have no adequate metaphysical ground for a normative morality, which "requires a criterion. . .adequate to account for the unconditional character of moral obligation. . . .What we call the moral law and values are abstractions which our limited and limiting minds make from the concreteness of the living God who is revealed."

Since this brief review is chiefly expository, I have refrained from commenting on many point concerning which I feel sure that most evolutionary naturalists would probably differ from the author.

S. J. HOLMES

NORTH AMERICAN PLEOSPONGIA. *Geological Society of America Special Papers, Number 48.*

By Vladimir J. Okulitch. The Geological Society of America, New York. $1.25 (paper). vii + 112 pp.; 18 plates. 1943.

Okulitch's monograph is an important step toward a fuller understanding of the nature of the exquisitely filigreed goblets from the Cambrian rocks, known to most geologists as the Archaeocyatidae or Pleospongia, even though the mystery of their origin and even of their biologic relationship is by no means entirely solved. Through recent researches these extinct organisms, typified by the genus *Archaeocyathus*, have grown in our understanding to the dignity of an extinct class of sedentary marine organisms. Okulitch (1937) suggested calling them Pleospongia, "to replace the indefinite term Archaeocyathi." He justifies this change because of the unanimity of the contemporary

leading students of the group in rejecting the early belief in their affinity to corals, and in accepting their biologic relation to sponges, and particularly to calcareous sponges, as Okulitch concludes. While the arguments against a coral affinity of the Archaeocyathi, which he so well summarizes, are quite convincing, the suggested introduction of the class name Pleospongia in place of Archaeocyathi, through attractive phonetically, does not seem necessary taxonomically. Besides, the neutrality of the older term is an asset rather than a liability, and will continue to appeal to prudent scientific minds, a reminder of the fact that we are a long way yet from a wholly satisfactory biologic explanation of this as well as of many other fossil organisms. For instance, in the generally satisfactory formal classification of the Archaeocyathi, as worked out largely by Bedford, Vologdin, and Okulitch, we find a strange "Subclass Exocyatha," with its three families embracing five genera, whose suggested relationship to the rest of the class is biologically puzzling and therefore taxonomically quite arbitrary.

The opinions of the authorities on these growth forms, which are treated as "Exocyatha," differ widely. They develop as encrusting protean structures, which in details of anatomy resemble the ordinary cup-shaped Archaeocyathi, but depart radically from their rigid symmetrical shape. Some authors (Bedford) treat Exocyatha as an independent organism parasitic on ordinary Archaeocyathi, others (Taylor) believe that they are specialized structures of the latter for the purpose of anchoring them to the substratum; now Okulitch boldly suggests that they are pathologic outgrowths, somewhat comparable to cancer development in higher animals, but with a by far more vigorous and even "independent growth." To quote Okulitch's own words in this extraordinary explanation: "I believe that exothecal tissue is an outgrowth of central more regular archaeocyathid, that such tissue was capable of *independent growth* and secretion of skeletal elements, essentially similar to skeletal tissue of parent, and that main differences are due to lack of architectural boundaries, which were imposed on regular forms but not on the excrescences. It is quite likely that if a regular archaeocyathid became surrounded by its own exothecal tissue it might have become *stifled by it and died* for lack of food and water circulation. Thus great prolificity of growing pleospongian tissue may have contributed to their eventual extinction" (p. 84, italics added). Elsewhere he states that Exocyatha "may be a pathologic condition somewhat akin to cancerous growth of higher animals."

Perhaps some biologists will immediately dismiss the whole explanation as a flight of fantasy, but the general merit of Okulitch's work justifies an attempt at critical analysis and understanding of the facts and reasoning behind it. The striking similarity of the microstructure of the protean encrusting "Exocyatha" to the regularly cup-shaped Archaeocyathi upon which they grow seemingly indicates a common biologic nature, and obviously inspired the view that in this growth the organism suddenly freed itself from the "architectural" bonds of its radial symmetry, so distinctly obligatory for all other "sub-classes" of the Archaeocyathi. This radical change from a rigidly symmetrical to an entirely protean growth-form reminds Okulitch of a pathologic outburst of growing substance, completely irregular and lawless, like cancer growth in higher animals, and, like the latter, fatal to the parent organism, and even to the whole race, which suddenly became extinct sometime in middle or, possibly, early late Cambrian time. It is an alluring hypothesis that seemingly explains at one stroke both the peculiar asymmetrical encrustration and the cause of extinction of the whole class.

There seems no serious objection against a possibility that pathological growth in organisms is as old as life itself, even though John M. Clark strongly insists on the secondary nature and gradual development, mostly in post-Cambrian time, of "Organic dependence and disease" (1921). It seems even quite defensible philosophically to expect, as I do, a more nearly universal symbiosis of organisms in their earlier than in their later geological history, whenever conditions of life were crowded and an intense struggle for living space existed. Such were apparently the conditions of benthic life, which left evidence in the form of immense bio-topographic structures known as reefs; and it is known that Archaeocyathi are found in a great number of individuals in reef-like accumulations. It is also known that various sessile invertebrates (foraminifera, bryozoa, sponges), and plants (algae) incrust each other in a variety of complex combinations; such are occasionally mentioned by naturalists but perhaps as yet not studied in as much detail as they deserve. In such intimate symbiotic combinations it is sometimes not easy to draw a boundary between one and the other of its biologic components; some calcareous algae resemble encrusting foraminifera macroscopically and microstructurally so much that only a trained eye can distinguish one from another.

In view of this it seems that in the case of the fossil Exocyathi an assumption of their organic independence from the rigidly symmetrical cups of the Archaeocyathi, which they occasionally so completely encrust, would be more acceptable to most biologists than the hypothesis that they are immense pathologic outgrowths, a view which seems to suggest that asymmetrical and unruly Archaeocyathi were merely mutations or saltations (perhaps the macro-mutations of Goldschmidt) from an original symmetrical stock. Perhaps like a strangler fig that may occasionally strangle even one of its own kind, a strangler Exocyathus could have grown over some of its symmetrical and rigid relatives which provided it with a desirable support, and the latter could have been occasionally

or even regularly strangled. On the other hand, the microstructure of the Exocyathi may be only "mimetic" of that of the Archaeocyatidae, and they could therefore belong to different kinds of organisms.

The Archaeocyathi are world-wide common Cambrian fossils which have been studied attentively only in the last fifteen years; there is apparently a great deal more to learn about them than has been revealed to date. Okulitch's monograph is well organized and scientifically balanced; the previous literature is covered exhaustively and critically; not only the morphology and anatomy, but also the ecology, ontogeny, and biological relationships of the fossil are described and discussed. Though most illustrations are attractive and clear, a few (photographs of *Mattewcyathus*) are illegible. Standardization of magnification might be recommended in the further studies, which, we may hope, will follow.

M. K. ELIAS

OZARKIAN AND CANADIAN CEPHALOPODS. PART I: NAUTILICONES. PART II: BREVICONES. PART III: LONGICONES AND SUMMARY. *Geological Society of America Special Papers, Numbers 37, 49, and 58. By E. O. Ulrich, Aug. F. Foerste, A. K. Miller; with W. M. Furnish (I); with A. G. Unklesbay (III). The Geological Society of America, New York.* I, $2.00 (paper); II, $2.50 (paper); III, $2.50 (paper). I, x + 157 pp.; 57 plates. II, x + 240 pp.; 70 plates. III, x + 226 pp.; 68 plates. 1942; 1943; 1944.

Behind the somewhat dry monotony of the technical description of many genera and species of the earliest undoubted fossil cephalopods, there lies the important scientific and human drama of a one-man crusade for recognition, besides the universally accepted six, of two more Paleozoic geological systems: the Ozarkian and Canadian. Not since the "Sturm und Drang" days of the creation of modern geo-chronology, when the Cambrian, Silurian, Devonian, and Carboniferous periods of the Paleozoic were established, and to which somewhat later the Permian and Ordovician were added, has a more serious attempt been made at a revision of the whole concept of the Paleozoic era. With a patience and zeal seldom rivaled in the history of the geological sciences, E. O. Ulrich, with occasional help from associates and friends, has spent a good half of his lifetime in accumulating bit by bit geological and paleontological evidence indicating that between the two oldest Paleozoic systems, the Cambrian and the Ordovician, there are two other systems, fully equal to them in magnitude of time, and characterized by some marine organisms peculiar to them. In 1911 came a summation of geological evidence for the existence of the Ozarkian and Canadian periods, the ambitious "Revision of the Paleozoic Systems" published by the Geological Society of America; and in 1938 the same Society published the splendid monograph on the "Ozarkian and Canadian Brachiopoda," by Ulrich and G. Arthur Cooper. This publication broke through the sceptical resistance of Ch. Schuchert, himself a master in brachipod studies and Cooper's teacher, and convinced him of the existence of a Canadian period of time as a major chapter in the evolution of articulate brachiopods. Said Schuchert in his review (1939) of Ulrich and Cooper's brachiopod monograph: "On the basis of brachiopods, a good case can be made out for a Canadian system"..."with at least 31 genera..., of which 23 genera [are] restricted to this system."

In 1940 another of Ulrich's prominent opponents came out with this statement: "The establishment of the Ozarkian System as an independent system is the monumental achievement of Dr. Ulrich, and that he has triumphantly maintained it in spite of all opposition is a tribute to Ulrich's scientific acumen. As one of his former critics I take pleasure in acknowledging my conversion to his 'Ozarkian doctrine'" (Grabau, *The Rhythm of the Ages*, Peking, 1940). He also endorsed Ulrich's Canadian system.

Death claimed Ulrich on February 22, 1944, when the third and last volume of the *Ozarkian and Canadian Cephalopods* went to press. His close collaborator Foerste died in 1935, and A. K. Miller and his pupils and associates W. M. Furnish and A. G. Unklesbay shouldered the principal work of systematic description for the three volumes of the cephalopod monograph. The scope and importance of the work may be judged by the fact that the known number of species of Ozarkian and Canadian cephalopods of the world has been trebled: 287 new species are described and 126 previously known are revised, the whole being lucidly illustrated on 195 plates.

A reader who is accustomed to astonishing and bizarre forms from the far-away world of the geological past will be disappointed with the Ozarkian and Canadian cephalopods. Their shells, which are the only parts preserved as fossils, are simple septate tubes with little or no ornamentation. The cephalopods' great diversity of form, internal complexity, and fancy ornamentation came about much later in the Paleozoic and the Mesozoic. There are few features on which the classification of their early forms can be based: shape and size of conch, slight differences in septa, reflected externally in suture lines of the internal molds, and particularly structure of the siphuncle are the principal characters used in taxonomic distinction. By the combination of these are recognized species, genera, and families, which probably are not natural groups, but nevertheless permit orderly description of the fossil material. Equally arbitrary is their segregation into three major groups, the Brevicones, Longicones, and Nautilicones (a separate volume of description being devoted to each); but this grouping

is practical and finds immediate justification in the chronological order of their appearance in the geological systems discussed.

The coiled Nautilicones appear in a great number of genera and species suddenly in the upper Canadian, as if the ability to coil originated as a mutation independently in several phyletic lines, or proved immediately so useful that coiled forms spread and evolved rapidly in the late Canadian seas; it seems that a combination of both causes was probably instrumental in their evolution.

Some of the numerous more or less curved Brevicones of the early Canadian apparently mutated into Nautilicones. The ability to curve slightly had been acquired already by the rare late Cambrian forms, as the earliest undoubted cephalopod *Plectronoceras* (originally described by Walcott, 1905, as *Cyrtoceras cambria*) indicates. The straight and curved Brevicones are taxonomically divisible into two major groups, "the Piloceratidae which is characterized by its large size and holochoantic siphuncle, and the Cyrtendoceratidae in which typically the conch is much smaller and the siphuncle is relatively slender and is orthochoantic in structure." The small Cyrtendoceratidae evolved into numerous forms in the course of Ozarkian time and reached their acme of development in the upper Ozarkian. Only a few Piloceratidae are known from the Ozarkian, mostly from its upper part, and they evolved into a sizable group only in the Canadian period. The long, slender, straight, and slightly curved Longicones are equally characteristic of the Ozarkian and Canadian and were cosmopolitan in distribution.

When reviewing *Ozarkian and Canadian Brachiopoda*, Schuchert pointed out the peculiar occurrence of by far the greatest number of species and genera of brachiopods in upper Ozarkian and upper Canadian times. The same is true for the Brevicones and Longicones, while Nautilicones were found exclusively in the upper Canadian. It seems too early to speculate on the cause of this fact: shells of both brachiopods and cephalopods of the Ozarkian and Canadian are comparatively so simple, and parallel evolution in their phyletic branches is so obvious (see Ulrich and Cooper, 1938) that restoration of the evolutionary sequence of their recognized genera is at the present time most difficult and has not been attempted for either the brachiopods or cephalopods.

Someone with stamina, patience, and enthusiasm equal to those of Ulrich should concentrate on the additional determined collection of a larger number of these fossils from their most prolific localities, in order to elucidate their evolution. Ulrich and his associates have done splendid pioneering in the establishment of an orderly succession of remains of invertebrates in the early Paleozoic rocks of America that justifies the validity of the Ozarkian and Canadian as major units of geological time.

It is a challenge to and an obligation of the new generation of geologists and paleontologists to do a better and more complete work on these rocks and their fossils.

M. K. ELIAS

GENETICS AND CYTOLOGY

THE DICE OF DESTINY. *An Introduction to Human Heredity and Racial Variations.*

By David C. Rife. Long's College Book Company, Columbus, Ohio. $1.75. 163 pp. 1945.

Rife has written a simple and direct exposition of the nature of individual and racial human variations, leaving out of his account all such non-essential concepts as chromosomes and linkage. This unusual approach has avoided those genetic technicalities that appear so formidable to laymen.

The book begins with an explanation of the inheritance of simple human hereditary traits, such as the blood groups and tasting ability for phenyl-thio-carbamide, then takes up more complex characteristics, such as hair color, eye color, skin color, and stature; sex and associated traits; and, at some length, twins, dermatoglyphics, and handedness, subjects of Rife's own investigations. After a central chapter on the interaction of heredity and environment, the author considers mental capacity, special abilities, personality, and success or failure in life. Chapters on Race, and Genes and Democracy complete the discussion. Rife has for the most part expressed himself clearly and vigorously, in a way that will appeal to his readers. The book is sadly marred by numerous typographical errors.

There are certain lapses in precision of statement. For example, Rife begins his chapter on Heredity and Environment by saying, "The language one speaks is solely a matter of environment.... One's blood group is determined solely by heredity." These statements help to perpetuate in popular thought that very dichotomy between heredity and environment Rife himself clearly wishes to avoid. After all, the capacity to learn and speak a language—any language at all—is genetically determined, and the first statement therefore cannot be strictly true. And there have already been many instances, such as that which Rife points out elsewhere of diabetes mellitus (in error confused with diabetes insipidus in the text), where "solely hereditary traits" have become through discovery shifted to the class of the environmentally controllable. The real point is that no phenotypic trait develops except in an environment of some sort, and that therefore all are potentially modifiable, even if only in a lethal direction. These criticisms would be unfair of a book with the aim of this one, were it not that general understanding of the real nature of the relation of

heredity and environment is so fundamental and so widely misunderstood. Rife has himself said in another passage:

> If a trait is shown to be hereditary, the attitude all too frequently is that nothing can be done about it. Conversely, if a trait can be altered by environment, it is usually assumed that heredity has nothing to do with it.

Yet he has himself perhaps contributed to further misunderstanding in this regard, from his very choice of title. *The Dice of Destiny* is a striking phrase that will leave many a reader, I am afraid, with a sense of the irrevocable and irremediable nature of heredity, in spite of all the author's cautioning.

With the author's views on eugenics most geneticists are likely to concur, although some will question the advisability of a general sterilization of morons, for which he argues. With his views on race more will differ. There are several controversial issues here. One arises from the statement that "the genetic differences between human races are similar to those between breeds of animals." This is true and not true—true if referred simply to the fact that differences in gene frequencies exist in both; not true in the sense in which so many will unfortunately take it, that human races are as dissimilar as the breeds of animals that are the product of artificial selection. Rife, of course, recognizes the action of natural selection in the one case and of artificial selection in the other, but he does not apparently consider the vast differences, both quantitative and qualitative, in their effects. One aspect of this question has already been more than adequately discussed by Dobzhansky in another review of this book (*Science* 102: 234. 1945). The argument that "because psychic traits in animals are rather rigidly determined by heredity they are so determined in man" seems indeed weak if natural selection in man has worked steadily for the most adaptable and modifiable type of genetic determination of intelligence and personality, while in other species it has not.

Another ground for differing with the author lies in the reasoning that because the genetic basis of racial and individual differences is the same, therefore "evidence of genetically determined mental differences must of necessity be considered evidence of similar differences between racial groups." It does not necessarily follow that because blood groups are inherited, the frequencies of the blood groups are different in different races. It happens to be true, but it had to be separately demonstrated. No matter how probable it may be that races differ in the frequencies of specific genes affecting mental differences, it remains to be proved that they do so differ, and further that, even if they differ, such differences are not offset in effect by complementary differences. There are many well established cases where even different species possess the same or very similar phenotypic traits that depend upon quite diverse genetic bases.

Finally, the view that "the greater the heterogeneity of a population the greater are the difficulties in attaining and maintaining democracy" is certainly questionable unless carefully qualified. It can be equally well maintained that, up to a point, civilization, and with it democracy, depend upon human diversity and non-homogeneity. Certainly such a view as that quoted above has no scientific validity, and its statement by a scientist as simple fact rather than as personal opinion may be vastly misleading.

Perhaps the best recommendation of this book is that it arouses such discussion. There is no doubt whatever that after the extreme views that have been expressed in recent years as to the absence of genetic racial differences in man, a book of a more conservative standpoint, such as this, has been sorely needed.

BENTLEY GLASS

A STUDY OF SPONTANEOUS MUTATION. *University of California Publications in Zoology, Volume 49, Number 10.*

By Richard B. Goldschmidt, with the collaboration of Richard Blanc, Werner Braun, Mary M. Eakin, Ruth Fields, Aloha Hannah, Leonie Kellen, Masuo Kodani, and Claude Villee. University of California Press, Berkeley and Los Angeles. $3.00 (paper). Pp. 291–550; plates 23–30. 1945.

This is the long awaited detailed report of data on mass mutation to which Goldschmidt has referred in shorter communications over a period of years. Consisting of some 125,000 words with 153 tables, many of them full page, and 98 figures of wing mutants and salivary chromosomes in both text and in additional plates, it is matched in extent only by the tremendous volume of work done by the senior author and his collaborators.

A careful reading of any part requires constant reference to the tables and a check of the material presented in other sections. Few readers will have either the time or the patience to read the entire treatise. The reader with little time at his disposal may get the gist of the facts presented, and of Goldschmidt's interpretation of these facts, by reading carefully the introduction, pp. 291–292, and the discussion and summary, pp. 524–530.

The bulk of the paper may be divided into a section dealing with spontaneous mutation, largely in a stock of plexus flies (pp. 293–297 and pp. 471–524), and a section dealing with the details of phenotypic expression of certain multiple allelic series mainly at the silver, arc, and plexus loci, factor interaction, modifiers, and chromosome aberrations affecting this expression. The first of these sections presents the

details which most geneticists interested in spontaneous mutation will want to study.

There seems little doubt from the facts reported here that the plexus stock has shown a uniquely high mutation rate at the silver, arc, and plexus loci, these mutations being in the form of a complex allelic series at each locus, and consisting in some cases of reverse mutation to or toward the so-called wild-type. Furthermore, these and a few other loci, including rudimentary, have mutated in a non-random manner, so that several mutations appeared either simultaneously in a pair mating or in the course of a few generations in a carefully pedigreed line. No explanation in terms of contamination or by mutation of a single suppressor having action at several loci can explain the facts. It is true that a valid scheme which spreads the occurrence of the mutations over a somewhat longer series of generations that Goldschmidt has postulated can be presented. But this in no way invalidates the main argument for the high mutation frequency at the several loci involved and for the non-random distribution of these mutations in time and in certain lines. In general, these findings are in line with recent work on high spontaneous mutation frequency in certain lines and particularly at specific loci, as reported by several investigators (Berg, Demerec, Neel and Spencer). It is also shown that the plexus line and its derivatives produce frequent small chromosome aberrations in the form of deficiencies, principally in the X-chromosome, and corresponding translocations to the 2nd and 3rd chromosomes. While these deficiencies and translocations seem in some cases to have arisen at or near the same time as the mutant alleles at mutating loci, they do not cover these loci. However, their presence in conjunction with the mutants results in peculiar ratios, particularly of sex-linked mutants in certain out-cross and back-cross tests.

Goldschmidt interprets these findings to mean that the mutation process is one dealing with the mechanical breakage of chromosomes, sometimes breakage on an intra-salivary band scale, in contrast to an interpretation of point mutations as chemical changes in genes. It should be stated that the mutants in question can be dissociated from the chromosome aberrations by crossing-over and then behave as typical point mutations. It would seem that the demonstration that certain cases of deficiency are of intra-band magnitude would leave the ultimate answer on the nature of point mutation to a time when analysis at a new level can be achieved.

The section on factor interaction and phenotypic expression presents in great detail a study of the several alleles, in heterozygous and homozygous form, at the silver and arc loci, the interaction of these compounds at the two loci, and epistatic effects when in combination with other mutants, particularly plexus. As both allelic series are extensive and both act on wing shape, it is not surprising that a complex array of phenotypes has been produced. Certain of these compounds and permutations are extremely susceptible to modifier action and therefore serve as excellent tools for testing for the presence of genetic variability in out-crosses to various wild-type and mutant stocks. Often the phenotypic expression is non-predictable in simple additive terms. Perhaps the chief value of this work has been the discovery of a background of genetic variability which would have gone undetected in the absence of the sensitive markers used. The work would have added value had it been done under carefully controlled environmental conditions and had these been specifically stated. Temperature, moisture, food medium, condition for pupation, larval crowding, etc. have marked effects on the phenotype of such genetic combinations as are here described. Without control of these factors, the carefully recorded results in this long series of experiments could in many cases not be duplicated upon repetition. It is not unlikely that some of the unexplained variability in results in certain experiments is due to environmental as well as genetic variability. However, much of interest concerning factor interaction is amply demonstrated.

As Goldschmidt has published elsewhere and here repeats an explanation of Demerec's miniature case in *Drosophila virilis* as being due to factor interaction rather than to a frequently mutating gene, it is only fair to state that a careful reading of Demerec's papers on frequently mutating genes reveals that he has made the critical tests demanded by his hypothesis and that the factor interaction explanation is invalid.

Typographical errors, particularly in the tables, some contradictory statements, and the confusing use of the symbol for a mutant when the stock containing the mutant is really meant, add to the difficulty in reading this section.

In conclusion, it may be stated that the phenomenon of repeated and non-random mutation in a particular stock has been well demonstrated; the data add considerably to our knowledge of the mutation process; and the results could only have been secured by the laborious method of rearing over a period of years long inbred lines carried on by pair matings. No other student of *Drosophila* genetics has as yet carried out this type of experiment on so large a scale.

WARREN P. SPENCER

CONSTITUTION AND DISEASE—*Applied Constitutional Pathology. Second Edition.*

By Julius Bauer. Grune and Stratton, New York. $4.00. xiii + 247 pp. 1945.

In *To Begin With*, Raymond Pearl attempted to provide for young biologists a selection of essential reading. Among the books he included in his list was one by

Julius Bauer, *Die konstitutionelle Disposition zu inneren Krankheiten*, in order, he said, "that it may become even clearer that genetics has relation to medicine." Were this list to be revised today, the book here reviewed might well replace that earlier discussion of the same subject. Readers in this country should be expecially grateful for an English version.

Constitution and Disease is a briefer book than the original, but remains comprehensive in its consideration of the relations of genotype to disease. Its survey of the literature, up through 1944, is remarkably thorough. In an introduction, Bauer deals with the meaning of normality and abnormality, the causes of variability, and the insight into the significance of constitution in relation to disease afforded by the study of identical twins. The author proceeds to define "constitution" as the realization in the phenotype of the potentialities of the genotype, and discusses with abundant examples the significance of constitutional factors (read "genes") in the etiology of diseases, in the alteration of the clinical pictures and courses of diseases, and in necessitating modifications in treatment. Next Bauer considers the constitutional inferiority centered in certain organs or tissues such as nervous system, heart and blood vessels, and digestive tract. Racial differences offer further evidence of the widespread existence of differences in the superior or inferior general resistance to disease of specific organs or tissues. Two chapters deal especially with the integrative systems of the individual constitution, the endocrine and the nervous. In the former the relation of genotype to endocrine glands, and to ultimate characters either through the endocrines or directly, is particularly lucid. The treatment of the relation of the genotype, nervous system, and ultimate expression of characters shows that Bauer was a leader in psychosomatic medicine long before the coinage of that term made its concepts popular among medical men. The classification of normal and abnormal constitution is characteristic of the author's thought. While agreeing that many of the earlier classifications of constitution were over-refined, he believes there is sound evidence for the existence of certain endocrine types, certain neuropathic constitutions, an asthenic constitution, and various diatheses of significance. Besides these, he describes a "status degenerativus," which appears to be no simple nor readily defined constitutional type, but simply a consequence of the chance inheritance by a single individual of a number of detrimental genetic traits. In this respect it is to be contrasted on the one hand with the pleiotropic effects of a single gene, and on the other with the consequences of linkage in heredity.

A chapter on certain major diseases with chiefly constitutional etiology serves to illustrate the principles previously considered. Diabetes mellitus, obesity, essential hypertension, various anemias, leukemia, Hodgkin's disease, hemophilia, peptic ulcer, and cancer are among the conditions considered here. A final chapter emphasizes the importance in the "ars medica" of modifying treatment to fit the individual patient, due regard being had for his particular constitution. One passage should be taken to heart by all doctors:

"Sometimes the following objections can be heard: There is no difference in the treatment of obesity, whether or not its constitutional (hereditary) character is recognized, therefore it is unimportant to be familiar with the latter; the same should be true with regard to other hereditary diseases. The correct aspect of the matter, however, is different. The physician carrying out a treatment must know as much as possible about the etiology, pathogenesis, and consequences of a pathologic state that he is about to treat. Otherwise he deserves the title of quack rather than of doctor."

Space fails for the adequate discussion of this truly important book. It is, to be sure, not without its faults. Only a medical man will be able to read it without a medical dictionary at hand for frequent consultation. The style is heavy and didactic. Bauer often assumes far more fundamental understanding of genetics than most of his medical readers are likely to have. He often cites evidence without discussing it thoroughly. Consequently he will impress many as dogmatic and arbitrary in his opinions. This is the more to be deplored because such a book as this is greatly needed and should have the widest possible influence. In any case, let it be required reading for every geneticist and every medical student.

BENTLEY GLASS

HEREDITY IN DAIRY CATTLE. *Lessons in Breeding and Herd Development for 4H and FFA Dairy Clubs and Other Beginners.*

By James E. Russell. The American Guernsey Cattle Club, Peterborough, New Hampshire. $2.50 (free to 4H and Future Farmers of America members). xvi + 135 pp. 1944.

Written for the practical farmer at the most elementary level, this book should serve as a useful guide for the improvement of animal stock.

Russell addresses himself specifically to "4H and FFA Dairy Clubs and other beginners." He includes a brief and very general discussion of the theory of genetics; and while, this might be enhanced by some good diagrams of chromosome behavior, it must be conceded that he effectively makes the points he wants to make. References to text-books for further study of genetics are added. The main part of the book teaches by example, describing in detail four successful Guernsey herds and the procedure by which their owners built them up, and drawing practical conclusions from these "case histories."

EILEEN SUTTON GERSH

THE ARTIFICIAL INSEMINATION OF FARM ANIMALS. *Edited by Enos J. Perry. Rutgers University Press, New Brunswick, New Jersey.* $3.50. 265 pp. 1945. Another book for the practical farmer, this one deals with more advanced theory and practice. Chapters are devoted to detailed accounts of the reproductive system and principles of genetics (Mendelian laws, linkage, sex linkage, multiple factors, lethals). Here again a diagram of meiosis might be helpful, and in addition it would be well to stress the statistical nature of genetic ratios, instead of leaving the impression that they correspond exactly with actual results.

Besides chapters on such topics as breeding systems, selection of animals, general technique of insemination, control of disease, and the organization of breeding associations, there are special chapters by experts on the application of artificial insemination methods to cattle, sheep, horses, birds, and swine.

EILEEN SUTTON GERSH

GENERAL AND SYSTEMATIC BOTANY

SARGENTIA. V. FRAGMENTA PAPUANA (*Observations of a Naturalist in Netherlands New Guinea*). *By H. J. Lam. Translated by Lily M. Perry. The Arnold Arboretum of Harvard University, Jamaica Plain, Massachusetts.* $3.00 (paper). iv + 196 pp. 1945.

The author of *Fragmenta Papuana* is at present Director of the Rijksherbarium at Leiden, but in 1920–1921 he was botanist to the Van Overeem Mamberamo Expedition into the high mountains of Netherlands, New Guinea. Between 1927 and 1929 he published, in the Dutch language, a series of seven articles describing his experiences with this expedition, all of them in Natuurk. Tijdschr. Nederl.-Ind. In the course of her studies of the New Guinea collections made by L. J. Brass in the 1930's, Lily M. Perry soon realized the significance of Lam's papers for anyone dealing with the plant life of the interior of that little-known region. It is due to her initiative and industry that we now have excellent English translations of Lam's seven papers.

The *Fragmenta* comprise a sort of botanical journal of the Mamberamo Expedition. Lam's material collections, in the form of specimens and records, are of course extensive; but like all trained field men, he found himself with a wealth of general information about the vegetation of the country for which there was no place in his formal records. He gives us these observations in the *Fragmenta Papuana*, interspersed with accounts of travel, climate, animal life, and the native aboriginal population.

The first of the seven articles is introductory. It contains a brief history of the exploration of the Mamberamo River, an account of the personnel and itinerary of the expedition of 1920–1921, and a description of some further explorations in 1922. The second article is very short, and is devoted entirely to meteorological observations made on the journey.

Fragments 3 to 6 inclusive deal with sections of the journey which fall within an equivalent number of phytogeographic provinces. Number 3 contains "Impressions of the Lower Mamberamo Territory"; number 4, of "The Meervlakte and the foothills"; number 5, of "The north slope of the Central Mountain Range"; and number 6, of the country "Above the forest limits: Doormantop and its vegetation," The seventh Fragment records the author's personal observations upon the "Land and people of the Dika and Toli Valleys." In each section there are notes on travelling conditions, with personal impressions of landscapes and vegetation, often supported by pen and ink sketches. Of particular interest and significance are the author's descriptions of plant communities, habitats, and growth forms.

It is difficult to designate outstanding features of Lam's papers, for together they make a connected story in which the standard of excellence remains high. I venture, however, to point out some features that seem worthy of particular note. The botanical descriptions of landscapes are extraordinarily effective and clear. The author obviously is cognizant of the complex methods now commonly used by ecologists for describing plant communities; yet he adheres to "plain language," and achieves living descriptions that can without a glossary be read and understood by anyone. A second fetaure of unusual interest is in Fragment 6 (pp. 123–137 of the translation). Here are observations on the ecological adaptations of alpine plants in New Guinea, with a discussion of theories for their origin and development. Here also is a general consideration of the origin and affinities of the New Guinea flora. The descriptions of the strange peoples met with in the high mountain valleys (Frag. 6) make a rather thrilling final chapter. The explorers found themselves literally in a stone-age civilization, for the people had tools made only of stone, bone, or wood. Curiously enough, they turned out to be friendly beings, living in agricultural communities on high slopes that required to be terraced for cultivation. Their contacts with the outside world had been almost non-existent, but there is some evidence that they had a tenuous connection with lowland races. Lam presents interesting botanical evidence of the extent and nature of this connection.

The *Fragmenta* make splendid reading not only for the professional botanist, but for the vicarious general traveller as well. Some of the botanical descriptions are much too detailed for anyone who has not had personal experience with tropical floras, but these paragraphs can be skipped without loss to the structure as a whole. The papers could have been made somewhat more acceptable to the general reader if the author had segregated his annotations on plants and

growth habits in such a way as to make them instantly recognizable as such.

Lily M. Perry is to be congratulated upon the excellence of her translation. She has followed the Dutch text very closely, and has so arranged the legends for the text figures (reproduced directly from Lam) as to make them clear to readers of English. Lam's text references to his collection numbers and his citations of literature are retained throughout. The index to the translation is partially annotated by subdivision under various subject headings.

H. M. RAUP

THE STRUCTURE AND REPRODUCTION OF THE ALGAE. *Volume II. Foreword, Phaeophyceae, Rhodophyceae, Myxophyceae.*

By F. E. Fritsch. Cambridge, at the University Press; New York, The Macmillan Company. $12.00. xiv + 939 pp.; 2 maps. 1945.

That in England between the years 1935 to 1945 a manuscript of such magnitude and complexity could be so thoughtfully, accurately, and successfully prepared, that it should be so well edited, printed, proofread, and put together is nothing short of miraculous and reflects the highest possible praise upon its author, its editor, and the Cambridge University Press.

This second volume of Fritsch's monumental account of the primarily marine algae proves a fitting companion to the earlier one, in which an exhaustive account of the predominantly fresh-water forms was given. In format, it follows closely the first volume that, in turn, was patterned after Oltmann's classic *Morphologie und Biologie der Algen.*

After a Preface which outlines the scope of the volume and includes acknowledgments, there follows a Foreword. Here are found generalizations on the conditions of life, distribution, and special ecological characteristics of the habitats frequented by these algae. There is then included a discussion of the seasonal occurrence and geographical distribution of the marine forms. The latter account is supplemented by two most interesting maps, found in an appendix (not in the body of the text, as stated) in which are shown the geographical distribution throughout the world of certain seaweeds. The effects of differences in temperature, light, salinity, etc., on distribution are discussed, and striking instances of discontinuous ranges of species and possible endemism are cited. The general conclusion with respect to endemism is that ". . . our knowledge of distribution of marine Algae is still too limited to provide much positive evidence as to the existence of endemic genera and species."

The body of the text follows in the main the outline of the companion volume. Each of the classes is discussed primarily from a morphological and developmental standpoint, only the principal outlines of the taxonomy being considered. Such features as the vegetative construction, cell wall, chromatophores, special morphological features, cytology, and life cycles are described in great detail; and the whole is copiously and accurately documented throughout.

After the account of each class and order, there is a comprehensive list of literature to which reference has been made in that portion of the text. For so complex a work, this system of recurrent bibliographies no doubt facilitates locating a particular citation in the text and certainly has the sanction of the parent work *Morphologie und Biologie der Algen.* It does result, however, in lending a certain discontinuity to the volume as a whole, and some repetition—insignificant criticisms if in their present position the citations are more useful to the reader.

The figures, of which there are more than 2,000, are chosen to illustrate morphological features described in the text. These are taken from various authors and, considering the grade of paper available in these times, have been reproduced fairly well. Some, however, have suffered from too much reduction but are, nevertheless, informative.

Throughout the volume interesting material on the biology of the various forms is given. This is particularly well done in the Myxophyceae where, in a section on distribution, ecology, and biology, extensive information is given concerning the unusual habitats in which these algae are found. These include such diverse situations as thermal springs, frigid lakes of the Antarctic, the surface of black sulfide muds on solar salt works, inside the human mouth, etc. Their activities as lime-borers and lime-depositing agencies, as well as their better known role as symbionts associated with fungi ("lichens"), bryophytes, gymnosperms, and Protophyta are also discussed.

The volume closes with an index of authors cited in the text, bibliographical references being distinguished by boldface page numbers from those in the body of the text, and an index of contents some thirty pages in length. As previously mentioned, two maps are tipped in at the end.

Naturally, it is impossible for one unfamiliar with the field of algology to evaluate the contents. However, from the clear and concise language in which it is written, the skillful manner in which the material is organized, the reputation of the author as an algologist, one instinctively realizes that here is a classic which will be of inestimable aid to future generations of students of these most interesting organisms.

F. K. SPARROW

A MANUAL OF SOIL FUNGI.

By Joseph C. Gilman. The Collegiate Press, Inc., Ames, Iowa. $5.00. xi + 392 pp. 1945.

As stated in the preface, the present volume is an outgrowth of an earlier paper by E. V. Abbott and J. C. Gilman entitled "A Summary of the Soil Fungi." Because of the continuing demand for this paper by soil microbiologists, industrial mycologists, and medical workers, the junior author felt an up-to-date revision highly desirable.

The volume is, admittedly, a compilation of descriptions of soil fungi and "... is designed to place a tool in the hands of investigators that will enable them to identify the soil fungi..." For the most part the arrangement followed is that of Lindau in Engler & Prantl, *Die natürlichen Pflanzenfamilien*. The Phycomycetes and the large genera *Penicillium*, *Aspergillus*, and *Fusarium* follow the treatments of modern specialists. Agarics, soil-borne plant pathogens not yet reported directly from the soil, those in leafmold, decayed wood, or other materials not yet fully incorporated into the soil are excluded, as are the Actinomycetes and the carnivorous Zoopagaceae.

Following the preface there is an introduction, consisting of brief discussions of the functions of the soil, the role played by fungi in the biology of the soil, and their further role as airborne entities that may become serious factors in the contamination and subsequent deterioration of commercial materials. There then follow brief sections dealing with the nature of fungi, as well as their sexual and asexual methods of reproduction, and finally concluding with an analytical key to the classes, orders, and families of the fungi to be considered.

For each of the classes, orders, families, genera, and species there is a technical diagnosis and, where necessary, keys to the genera and species are supplied. Authorities are given, without citations, for genera and species. The principal synonyms of the species are included and, following the specific description itself, the countries in which the fungus has been recorded, together with references to pertinent papers. A noteworthy feature of the 133-page section on Phycomycetes is the 76-page treatment of soil-inhabiting Chytridiales, Blastocladiales, Monoblepharidales, and Saprolegniales, groups ordinarily omitted in most books on soil microbiology. Descriptions of the terricolous Ascomycetes occupy 20 pages; the Fungi Imperfecti, including forms with ascogenous stages, 192 pages; and the Mycelia Sterilia, two pages. There then follows a bibliography of 169 titles, a six-page glossary, and an index to the genera and species.

A highly valuable feature of the manual is that each generic description is accompanied by a small block figure illustrating the principal features of the genus. The utility of these drawings is marred in a few instances, however, by inaccurate copying and at least one error in identity. Thus, in Figure 16, from Ward, illustrating a chytrid, *Rhizophlyctis rosea*, the zoospores are shown emerging from a sporangium which, in the original paper, was immature. Figure 17, from Whiffen, of a polycentric chytrid, *Nowakowskiella*, is here shorn of one sporangium and is devoid of an operculum, important diagnostic features present in the original. Figures 19a and b, intended to illustrate the genus *Blastocladiella*, are in reality figures of *Blastocladia parva*.

The book will beyond question prove highly useful to the general soil microbiologist and others, as has been evidenced by the popularity of its shorter predecessor.

F. K. SPARROW

CATALOGUE OF THE VASCULAR PLANTS OF S. TOMÉ (*with Principe and Annobon*).

By Arthur Wallis Exell, and other members of the Department. The British Museum (Natural History). £1 10s. xi + 428 pp. 1944.

The vascular flora of three volcanic islands in the Gulf of Guinea is the subject of this paper recently published by the British Museum. Exell collected personally on S. Tomé, Principe, and Annobon in 1932 and 1933, visiting also the island of Fernando Po. In addition to his own collections, he examined most of the earlier plant records from the islands, specimens for which he saw in European herbaria. Thus the work achieves that degree of definitiveness that is possible only when a floristic student has access to original collections and types.

Most of the book is devoted to the critical catalogue of the flora, wherein there are the necessary synonymy, citation of specimens, geographical and ecological notes, and discussions of imperfectly known species. The text for a few of the families was prepared by specialists, but most were done by Exell himself. Totals of 36 new species and 4 new varieties are described, many of them illustrated by line drawings. In addition 35 new names or new combinations are proposed.

Some 55 pages at the beginning of the book are devoted to two chapters dealing first with the geography and geology of the islands, a short history of their botanical exploration, and descriptions of the general aspect of their vegetation. Second, there is a review of the phytogeographic origins and affinities of the flora. The islands prove to have a prevailing floristic relationship with the neighboring coasts of Africa, but they demonstrate also the effects of insular isolation and endemism. Endemics on one or more of the three islands total 171 among the 683 species regarded by the author as geographically significant. He finds that "each island has (in addition to its endemic species) a comparatively large number of species found on the mainland but not on the other islands," and concludes that "this, again, seems strong evidence that the islands were never connected with each other, but that each received an independent

quota from the rich mainland flora." In the second chapter (pp. 48–49), there is a critical analysis of the sources of error that commonly rise when floras are compared statistically. This analysis could be studied with profit by anyone whose phytogeographic problems involve such comparisons.

Although the work is devoted primarily to the vascular flora, a list of the non-vascular cryptogams collected on Exell's expedition of 1932–33 is included as Appendix I. A second appendix gives a list of vernacular plant names used on the islands.

H. M. RAUP

THE ORCHIDS OF NEW SOUTH WALES.

By H. M. R. Rupp. National Herbarium, Sydney, N. S. W. 9s. (cloth); 6s. (paper). xv + 152 pp. 1943.

This book was issued by the National Herbarium at Sydney as a part of a projected "Flora of New South Wales." It contains descriptions of 252 species, representing 40 genera. In a few cases, varietal distinctions are made within the species. The paper is amply supplied with keys. Rather detailed descriptions are given for each species, and there are 23 plates containing excellent line drawings. Two new varieties are described by the author in the main part of the text. One is *Dendrobium Kestevenii* var. *coloratum*, and the other is *D. Kingianum* var. *pulcherrimum*. In an addendum, however, descriptions of five new species of *Diuris* are given. These were contributed by Mrs. Pearl R. Messmer and are as follows: *D. semilunulata, D. victoriensis, D. polymorpha, D. flavopurpurea*, and *D. lineata*.

For each species is given the date of the original publication and the principal synonymy. Wherever common names are available they are also included. The geographical distribution is first summarized, and then supported by the citation of specimens. In the summarized distribution four main divisions of New South Wales are used. These are based upon the major topographic provinces: coastal, tableland, western slopes, and western plains areas. They are looked upon as natural divisions, whose growing conditions reflect differences of climate. No map of these provinces is included, but the student is referred to a paper by Anderson on "Trees of New South Wales" (1932), where such a map is published. The book closes with an explanation of specific and varietal names, a general glossary of botanical terms commonly used in the paper, and an index to species and varieties which contains also author citations.

The introduction to the book deserves some special attention. It does much to make the book useful to the amateur botanist, whereas the professional taxonomist would be content with the bare taxonomic and geographic treatment. The author gives a well written and clear description, illustrated by sketches, of the principal parts of an orchid flower, followed by a brief discussion of other characteristic features of orchid life. The history of orchidology in New South Wales is adequately summarized. Advice is given on methods of collecting, in a brief section which contains also some very interesting and significant notes on local distribution and habitat selection among the orchids. These notes might well have been enlarged upon and made into a very useful addition to the whole discussion. It is obvious that the author speaks from wide experience in the field, and, as is so often the case among floristic students, the common facts about places in which plants live are either passed over as of little importance, or are considered to be of such general knowledge as to be unworthy of mention.

H. M. RAUP

FLOWERS IN BRITAIN *Wild, Ornamental and Economic and Some Relatives in Other Lands.*

By L. J. F. Brimble. Macmillan and Company, London. $4.50. x + 393 pp.; 18 colored plates. 1944.

Flowers in Britain is a non-technical book concerned with wild and garden flowers and economic plants of the British Isles. In the words of the preface, the book is "written for anyone who is interested in or wants to know something about flowering plants. It is not intended solely for the professional botanist, and therefore no previous knowledge of botany is assumed."

It is impossible for an American who is unfamiliar with the British landscape to examine critically the actual floristic content of the work. The method of presentation, however, is intriguing and worthy of comment. Floristic books, particularly in America, usually run to one or the other of two extremes. They are either as dry as the specimens which the conscientious botanist studies before he prepares the skeletonized and uninspiring, although technically correct work; or they are reduced to poetic or insipid descriptions of the beauties of Nature. Somewhere in between these extremes there ought to be a golden mean, and Brimble gives evidence of having come close to it.

The book begins with three introductory chapters which tell the reader what a flowering plant is and give a general survey of the significance of flowering plants in the development of modern civilization. Then comes a chapter on flowers: how they are made, how they vary, and how they function. In the preface the author has already placed high in the list of basic requirements for the beginning botanist that he learn to see how plants differ from one another, how they are related, and what their names are. The third chapter enlarges a little upon the idea of classification

and explains how it has come about. Here the author explains in a few simple sentences how he has organized the material of the book and why. The remainder of the text is arranged in chapters dealing with single families or small groups of families of flowering plants. The order of treatment is taken from Hutchinson's *Families of Flowering Plants*, and begins with the buttercups.

Within the treatment of each family are three subdivisions: first is a description of the wild species; second, a brief treatment of the species of the family which are commonly cultivated as ornamentals in English gardens; and third, some account of the economically valuable plants which come from the family. All this is done in simple but delightful English. There are no keys, but a few descriptive phrases are usually slipped in to make possible the separation of common species within the genus. Common names are given in boldface type, with Latin names italicized and in parentheses. For identification the author further relies upon habitat notes, and upon habit photographs and colored plates. There are 17 of the latter, each consisting of several colored drawings and inserted in its appropriate place in the text. The pictures are small, but nonetheless effective for the recognition of common plants.

The book is delightfully readable, and numerous bits of verse scattered through it are selected with unusual perspicacity. Not the least of the attractions are the short descriptions of plants useful to man. In them the author has tapped a seemingly inexhaustible well of human interest—one that is apparently unknown to most authors of floras, popular or otherwise.

I hope the author will not be offended if we in America find a little amusement in what he says about Indian corn. Having had some experience with British ideas about corn, I turned eagerly to see what Brimble would have to say on the subject. On page 375 is the following gem:

"**Maize** or **Indian corn** (*Zea mays*) is now cultivated throughout tropical and sub-tropical regions, though it is also grown in certain temperate areas. It is a native of Mexico. It is indeed a useful plant for the grain is converted into flour by grinding, it is also eaten without grinding, and in this form when dry fed to poultry; the green cob is a very favourite vegetable; the leaves are used as fodder; the spathes surrounding the unopened spikes are used in papermaking; and the dry cobs are used for firing."

Not a word about the Missouri Meerschaum!

H. M. RAUP

A BOOK OF WAYSIDE FRUITS.

By Margaret McKenny and Edith F. Johnston. The Macmillen Company, New York. $2.50. 78 pp. 1945.

In this unusually attractive little book the author and her artist have chosen for description and illustration about 33 varieties of familiar and beautiful fruiting plants commonly seen along the wayside in early summer, midsummer, and autumn. The brief description of each plant included in the text is accompanied by a full-page color plate, very well executed, and illustrating not only the fruit but the flowers and foliage as well. Those who are familiar with the companion volumes on garden flowers and wild flowers previously published by the author and the artist will welcome this new addition.

POISONOUS PLANTS OF HAWAII.

By Harry L. Arnold. Tongg Publishing Company, Honolulu, Hawaii. $2.00. 71 pp. 1944.

This little book, although it contains but two chapters and covers only 71 pages, apparently deals rather thoroughly with the poisonous plants of Hawaii. In the first chapter attention is given to the common and truly dangerous plants, of which there are less than a dozen. The habitats, common names, and recognition characters are given for each species, together with its poisonous principles, the symptoms of poisoning from them, and antidotes with which to combat the effects. The second chapter is devoted to a comprehensive plant list in which are described "all of the plants which have been listed by various authors as being poisonous, followed by as accurate information as is available regarding their distribution and their poisonous nature." Some of these have proved non-toxic, while others are either uncertainly or only slightly poisonous, or so rare as to make them of no great significance. The book closes with a bibliography of 48 titles and an index to both common and scientific names.

There are 24 plates containing excellent line drawings, all of them reproduced from Otto Degener's *Flora Hawaiiensis*. The author also credits Degener with the solution of intricate problems of taxonomy and nomenclature involved in the work.

The book is clear and in general well written. One is impressed with the small number of plants in Hawaii which are actually dangerous to man or his domestic animals. They are all apparently easily recognizable, and this book should do a great deal to make them of common knowledge and thus to reduce the number of cases of poisoning.

H. M. RAUP

ECONOMIC BOTANY

THE ENCYCLOPEDIA OF FRUITS, BERRIES, AND NUTS AND HOW TO GROW THEM.

By Albert E. Wilkinson. The New Home Library, The Blakiston Company, Philadelphia. 69 cents. ix + 271 pp. 1945.

This small volume, scarcely comprehensive enough to be called an encyclopedia, is divided into two parts. The first treats in alphabetical order the familiar as well as many of the exotic fruits, berries, and nuts which are grown in the United States and other parts of America. The largest amount of space is devoted to the more important crops, such as apples, peaches, plums, strawberries, and the like. These are discussed in detail from the standpoint of description, types, varieties, soil, climatic requirements, planting, cultivation, fertilizing, pruning, insects, diseases, and propagation. Common or botanical names by which fruits, berries, or nuts are known are listed alphabetically with cross-references to the name, usually a common name, under which the descriptive material is found. Some of the botanical names, unfortunately, are not correct.

The second part of the volume is a gardener's guide which brings together general instructions and suggestions applying to the fruit garden as a whole. Recommendations of varieties and planting arrangements are given for various regions of the United States in considerable detail.

The book is obviously intended for the small home gardener whose botanical and horticultural knowledge is limited. For this purpose it should prove useful.

P. C. MANGELSDORF

GREEN CARGOES.

By Anne Dorrance. Doubleday, Doran and Company, Garden City, New York. $2.00. 187 pp. 1945.

Reading in this book about the transport of seeds and plants about the world, from the time of the Phoenicians to the present day, has a great deal in common with walking through a rambling old garden with a guide. Here is a peach trained on the wall, here are fragrant lavender and allspice, here a stalwart row of tulips. Some of the plants are native Americans; others have become "naturalized," and we hear of their origins and the sometimes hazardous voyages which brought them to different parts of the world. Not that the book is confined to garden plants—economic crops such as rubber, quinine, and cereals are also considered.

The profusion of interesting stories makes this an entertaining little book, though its usefulness in other respects is limited, the more so by the absence of any index of plant names.

EILEEN SUTTON GERSH

GENERAL AND SYSTEMATIC ZOOLOGY

THE MARINE ANNELIDS OF NORTH CAROLINA. *Duke University Marine Station, Bulletin No. 2.*

By Olga Hartmann. Duke University Press, Durham, North Carolina. $1.00 (paper). 54 pp. 1945.

This clearly printed, clearly illustrated little volume promises to be so useful that it will be a "must" for anyone seriously interested in any kind of work involving any of the marine annelids of our Atlantic coast. Because it is close to Cape Hatteras, Beaufort, where this study was made, claims most, though not quite all, of the annelids found from Maine to Florida.

Each species is described briefly and many are illustrated in the ten full-page plates of line drawings. Synonyms, bibliography, distribution, and ecology are given with special notes about habitat, abundance, breeding habits, and salient features for tentative identification in the field. Under each family there is a key to the species. The list of ecological associations and habitats gives the species to be found in each. The usefulness of the book is further enhanced by a list of characteristic trails, surface marks, and tubes; an index; a bibliography; and maps of the Beaufort region.

This volume will not only delight the pure naturalist but win the gratitude of every experimentalist and ecologist, for it is evident that many old confusions have been cleared up, the known range of many species extended, more than one taxonomic misalliance corrected, and a clear, comprehensive, and useful picture of the group presented. Much remains to be done in this and other groups and it is greatly to be hoped that the Duke University Marine Station will issue subsequent volumes of a similar nature.

GAIRDNER MOMENT

BIBLIOGRAPHY OF BIOGRAPHIES OF ENTOMOLOGISTS. *The American Midland Naturalist, Volume 33, Number 1.*

By Mathilde M. Carpenter. The American Midland Naturalist, University of Notre Dame, Notre Dame, Indiana. 75 cents (paper). Pp. 1–116. 1945.

The title of this work clearly indicates its scope and usefulness. The author states in the introduction: "The references include not only obituaries, but birthdays, portraits, anniversaries, biographies and disposition of collections." The labor involved in the preparation of this extensive work must have been great and one wonders if it was worthwhile.

ROBERT MATHESON

COMMON INSECTS OF HAWAII.

By David T. Fullaway and Noel L. H. Krauss. Tongg Publishing Company, Honolulu, Hawaii. $3.50. 228 pp. 1945.

The title fully indicates the contents. The authors give an approximate census of the known insects of the Hawaiian Islands, indicating the number known to be endemic and those introduced. The first chapter gives a very brief account of insect structure and metamorphosis. The following eight chapters deal with the various orders, the authors selecting certain families and species for discussion. Every species discussed is illustrated on one of the plates. The size of each insect illustrated is shown by a line placed beside the picture, and this is a valuable feature. Unfortunately the colored plates are not all that they should be, but they give a fair representation of the form being discussed. The last chapter discusses the relatives of insects, such as the mites, spiders, scorpions, pillbugs, sowbugs, centipedes, and millipedes. These are illustrated in black and white. The book should prove of value to the people of Hawaii, as most of the insects described and figured are of economic importance.

ROBERT MATHESON

A GUIDE TO THE STUDY OF DRAGONFLIES OF JAMAICA. *Bulletin of the Institute of Jamaica Science Series, Number 3.*

By Francis Cecil Whitehouse. The Institute of Jamaica, Kingston. 1s. 6d. (paper). 69 pp. 1943. This is a list of dragonflies collected by the author between November 1941 and May 1942. To this list are added all species previously recorded from the island. The author gives notes on habits and habitats of the species collected, with brief characters for field identification and places of collection. The publication provides a brief handbook of the dragonflies of Jamaica.

ROBERT MATHESON

THE MOSQUITOES OF NEW JERSEY AND THEIR CONTROL.

By Thomas J. Headlee. Rutgers University Press, New Brunswick, New Jersey. $4.00. x + 326 pp. 1945.

T. J. Headlee is to be congratulated upon issuing a new and complete report on the mosquitoes of New Jersey, their biologies, habits, and habitats, and up-to-date methods of control devised through long years of experience. The original report by John B. Smith (1904) was a classic on mosquito biology and methods of control. Headlee has brought it down to date.

The opening chapter stresses the value of control, and the last chapter presents in summary form the increased valuation of taxable lands due to mosquito reduction. The other chapters bear the following titles: Structure, Classification and Keys; The New Jersey Mosquito Fauna; Mosquito Biology; Influence of Environment; History of Mosquito Control in New Jersey; Principles and detailed Procedure of Mosquito Control; Larvicides; Mosquito Repellents; Laws relating to Mosquito Control.

The titles clearly indicate the scope of the work. Chapter 3 consists of only a list, presented in tabular form, of the trap collections of female mosquitoes for the ten year period, 1932 to 1941. The chapter on mosquito biology, the longest in the book, is largely taken from the work of John B. Smith. The chapter on the influence of environment is restricted almost entirely to the conditions obtaining in New Jersey, with no mention of the extensive work done in this field by many workers in this and other countries.

In chapters 7 and 8 the reviewer had anticipated a fuller account of the principles of mosquito control, and of larvicides. Both chapters are brief and have but few illustrations of the machines and methods so successfully developed in New Jersey, except in some of the plates which are not referred to in the text. The discussion of the development of new machinery and its usefulness is very brief.

The illustrations, except the sixteen plates, are nearly all from the report of John B. Smith. Unfortunately there is no index, and the bibliography is very brief. The printing is well done, but the paper is not of the quality one could wish for such a work. There are a number of obvious errors, but these do not detract from the usefulness of the work. The volume should prove of great value to all those interested in mosquitoes and their control.

ROBERT MATHESON

STUDIES ON THE ANOPHELINE COMPLEX OF WESTERN AMERICA. *University of California Publications in Entomology, Volume 7, No. 11.*

By Thomas H. G. Aitken. University of California Press, Berkeley and Los Angeles. $1.25 (paper). Pp. 273–364. 1945.

This is an elaborate and detailed study of the anopheline complex of western America. No adequate summary can present these painstaking, detailed studies on the morphology and biology of the adults, larvae, and pupae of the species and varieties discussed. Each species is dealt with in great detail. The author concludes from his studies that the following may be considered to constitute the complex: *Anopheles maculipennis occidentalis, A. maculipennis freeborni, A. pseudopunctipennis franciscanus, A. pseudopunctipennis franciscanus* var. *boydi,* and *A. punctipennis.* Keys are given to the females, males, larvae, pupae, and eggs of the forms indicated above. There is an excellent discussion of the distribution of these anophe-

lines, and there are many illustrations supporting the views held by the author as to the relationship of these forms. An excellent bibliography completes the paper.

ROBERT MATHESON

A SYSTEMATIC STUDY OF THE PACIFIC TUNAS. *Fish Bulletin No. 60.*

By H. C. Godsil and Robert D. Byers. State of California Department of Natural Resources, Division of Fish and Game, Sacramento. Free upon request (paper). 131 pp.; 2 plates. 1944.

In order to clarify the differences among the tunas of the Eastern Pacific specimens were dissected and their morphology compared. Whenever possible, representatives from the Western and Mid-Pacific areas were similarly studied. It is concluded that the skipjack, the yellowfin, and the albacore of the Eastern Pacific are similar to those of Japan and the Hawaiian Islands. The bluefin tuna of Southern and Lower California are the same and are temporarily assigned to the same species as *Thunnus thynnus* of the Atlantic. Lacking Japanese material for direct comparison, the bluefin of Japan has been considered separable. The existence of a fifth species, here called the big-eyed tuna, is confirmed.

For each of the five forms, measurements are given in tabular form, with descriptions of the external appearance, the teeth, tongue, branchial assembly, viscera, skeleton, and excretory and circulatory systems. The appearance of the viscera *in situ* was found to be the most valuable and infallible character for identification; specific differences were also found in parts of the circulatory and excretory systems. Without further material, no nomenclatural revision was considered advisable; and in this respect, the work of Kishinouye has been followed.

HENRI C. SEIBERT

MODERN BIRD STUDY.

By Ludlow Griscom. Harvard University Press, Cambridge, Massachusetts. $2.50. x + 190 pp. 1945.

A lot of interesting and useful information gathered from the field of modern ornithology is presented here by the well known American ornithologist, Griscom. The first chapter traces the change of ornithological study from the days of collecting and classification to modern field investigations emphasizing the living rather than the dead specimen. The next chapter, on capacity and intelligence in birds, includes many examples of bird behavior that tend to prove a limited intelligence. Beyond this point, no other interpretations on the observations are submitted. Chapter III, on the adaptability of birds, provides examples to show that "birds run the entire gamut between extreme specialization and great adaptability." In the former category are birds that are being, or have been, extirpated through the agencies of man, with others that are suffering from competition by introduced species. To the second group belong those birds, such as the barn swallow and robin, that have become more or less domesticated. The interesting observation is made that in new and quickly exploited countries, like North America and Australia, several kinds of birds are approaching extermination, while in older countries, like Britain, practically all species continue to exist, though some may have been greatly reduced in numbers over a long period of time. The causes and routes of migration are discussed in two chapters, followed by four chapters on distributional problems in South, Central, and North America, areas with which the author has had the greatest experience. The final chapter is concerned with classification and speciation.

The book is an outgrowth of a series of lectures, with the subject matter somewhat revised and expanded. The presentation is informal and affords extremely pleasant reading. The limitation of subject matter is certainly the author's choice, but the reader should not be misled by the title into thinking that all or even a large part of modern bird study has been considered. In the discussion of subspecies, the statement that "in all scientific works and articles the correct subspecies name must be given" is certainly open to question, depending on the type of work presented. In describing the eastern and western flickers,—"one has yellow wing linings and tail feathers and red moustache stripes; the other has red linings and tail feathers and black moustache stripes"—the author has apparently made a slip, although one is led to believe that this error is duplicated previously where a red facial stripe is mentioned in connection with sex recognition. If reference is made to Noble's experiment (as indicated in the bibliography), the eastern species with black moustache is meant. The chapter on capacity and intelligence leaves the reader uninformed as to why birds act as they do, no mention being made of modern concepts of bird behavior. The reader, however, should not be deterred by these minor faults and personal criticisms, for this little volume is to be heartily recommended as worth-while reading for all students of birds.

HENRI C. SEIBERT

THE DISTRIBUTION OF THE BIRDS OF CALIFORNIA. *Cooper Ornithological Club, Pacific Coast Avifauna, Number 27.*

By Joseph Grinnell and Alden H. Miller. Cooper Ornithological Club, Berkeley. $7.00 (cloth); $6.00 (paper). 608 pp.; 1 plate. 1944.

This handsome and excellently printed volume was begun and half-way completed by the late senior author, while Miller has finished the task and revised the entire manuscript. It is an outgrowth of Grinnell's *Distributional List of the Birds of California* published in 1915. All species and subspecies "that are proved to have occurred naturally in the wild and within historic times in the State and of which at least one specimen has been preserved or verified as to identity by some ornithologist" are first listed systematically. This list, following the above criteria for inclusion, consists of 644 species and subspecies (427 species). An attempt has been made to standardize vernacular names by giving one name for the species as a whole and by adding a further descriptive name for the subspecies.

The main part of the volume gives, for each bird, its synonyms, status, geographic range, habitat, along with editorial notes. The synonyms apply only to names used with reference to California, listed chronologically, but with no citations. Under "status" are comments on seasonal occurrence, migration, relative numbers, and any definite changes in numbers and range recorded in historic times. The paragraph on range includes a complete and detailed listing, with references, of locality records, a certain amount of selection having been made in these records to avoid duplication. The notes refer mostly to nomenclatural departures from the A. O. U. check-list and to other taxonomic data.

Whenever more than one subspecies of the same species occurs within the boundaries of the State, a distributional map is provided showing the spots where breeding or resident specimens were collected. Because of the topographic variability of California, its proportion of geographic races is very high (fourteen subspecies of the song sparrow alone), and consequently the number of these useful maps is considerable. The supplementary list includes all those species that have been reported from California, whether correctly or incorrectly, but which do not pass the test as previously quoted. This list therefore includes sight records, introduced species, and those of doubtful occurrence. There is a complete index to bird names. This volume is indeed a fine piece of workmanship, showing the results of years of faithful amd careful compilation of records, and it will remain a standard reference for California ornithologists for many years to come.

HENRI C. SEIBERT

BIRDS OF GEORGIA. *A Preliminary Check-List and Bibliography of Georgia Ornithology. Sponsored by the Georgia Ornithological Society as Occasional Publication No. 2.*

Compiled by Earle R. Greene, William W. Griffin, Eugene P. Odum, Herbert L. Stoddard, and Ivan R. Tomkins, with a historical narrative by Eugene E. Murphey. University of Georgia Press, Athens. \$2.00. 111 pp.; 1 plate. 1945.

This publication is intended to serve as a preliminary check-list and bibliography of the birds of Georgia. To this end the five compilers have made an excellent contribution. Eugene E. Murphey has contributed a review of the history of Georgia ornithology. The bibliography is extensive and includes all articles dealing with or mentioning in part, birds studied or collected within the State. The birds are listed systematically, together with a brief statement on their abundance and distribution. The hypothetical list includes hybrids and forms of doubtful status. There is an appendix of Georgia bird clubs, a bibliography of local lists, and a map of the State with brief comments on its physiographic regions.

HENRI C. SEIBERT

BIRD PORTRAITURE. *"How to Do It" Series Number 35.*

By C. F. Tunnicliffe. The Studio, London and New York. \$3.50. 96 pp. 1945.

This is by far the most inspiring book on wildlife drawing that the reviewer has seen. Although designed for the beginner, an undeniably greater service has been rendered the experienced bird artist who needs exciting fare to incite him to try new ways of doing things. I, for one, expect that my own drawings will reflect some of the things Tunnicliffe has suggested. I showed the book to Major George M. Sutton, one of our "modern Audubons," and he was as enthusiastic as I. He said that although he had not thought of Tunnicliffe as one of Britain's best bird painters, he would unqualifiedly rate him so after glancing at this book.

Tunnicliffe advises the beginner to draw constantly from life, starting with domestic fowl, then working at the zoo. Some knowledge of a bird's skeleton and main feather groupings should be learned at this point. It is here that Tunnicliffe could have given the beginner more help by indicating in his sketches and diagrams some of the simple mechanical rules of bird feather arrangement that are so valuable to know—that most birds have twelve tail feathers, in two groups of six; that the central two feathers are on top and the overlapping is from the center outward. Also that on the upper surface of a bird's wing the primary feathers overlap on the front edges, while on the under-side of the wing they overlap on the rear edges. I had been drawing birds for several years before I learned that the median coverts overlap in the opposite direction to the secondary coverts.

Knowing these rules of thumb makes bird drawing much simpler.

Graduating from the barnyard and the zoo to quick-moving birds in the field is the next step. The author instructs the learner to "watch and watch and watch again. Do not draw while the bird is in view but try to get accurate impressions photographed on your mind, then sketch." He advises the careful study of birds found injured or dead, making measured drawings of upper and under tail surfaces, top and side views of beak, wing open, under surface of wing, etc. He suggests the careful study of some of the best bird artists, particularly the Chinese and Japanese masters, Audubon, and Liljifors the Scandinavian.

The book is lavishly decorated with fifty illustrations, many of them in black and white, some in color. These range from decorative spots to compositions where realism is the keynote. Several designs have an unmistakably Oriental flavor. I wish Tunnicliffe had told us precisely what medium was used in the various drawings and also something about the reproduction. He is obviously a master of wood-engraving, but one cannot tell whether some of the black and whites were done in this difficult medium or on scratchboard.

Bird Portraiture is a delightful book to own, to handle, and to read, not only for the artist and the student of birds but for anyone with curiosity and an eye for beauty.

ROGER TORY PETERSON

MAMMALS OF THE PACIFIC WORLD.

By T. D. Carter, J. E. Hill and G. H. H. Tate. The Macmillan Company, New York. $3.00. xvi + 227 pp. 1945.

This semi-popular small hand-book is issued under the auspices of the American Committee for International Wild Life Protection as one volume of a planned "Pacific World Series." Even though most our armed forces will soon return from the vast Pacific regions and thus lose interest in the details of nature peculiar to the Pacific World, this book will retain its value for many scientists as a convenient and authoritative compilation and condensation of the widely scattered literature on the mammals that live on the innumerable islands of the world's largest ocean.

After a brief introduction, dealing with the distinctive characters and the classification of mammals, the great numbers of mammalian species of the Pacific area are concisely described in taxonomic order and with the avoidance of technical terms and details as far as possible (generic names are given in parentheses, and family- and order-names are mentioned in sub-titles). A third, short section of the book is devoted to the distribution of mammals in the Pacific regions, with a brief discussion of the origin, evolution, dispersal, etc., of these animals. Four pages deal with the preservation, collection, and study of mammals. There follows a brief glossary of unavoidable technical terms and, finally, an alphabetical Index-Checklist of the mammals inhabiting each island or island group. This last part, comprising 38 pages, is particularly welcome for convenient reference. The authors mention, however, that this list may still be incomplete, "due to our lack of knowledge or collecting," and appeal to the reader to report any mammal encountered on a particular island that is not contained in this list under the name of the island. The 69 illustrations are adequate, but might have been more clearly reproduced.

A. H. SCHULTZ

THE SQUIRREL BOOK.

By Phyllis Kelway. Wm. Collins Sons and Co., London and Glasgow. 4s. 78 pp. 1944.

This is a delightful story of two wild squirrels, Yump and Nuffles, found when babies and raised to adulthood by the author. Her keen observations, her patience, sympathy, and real understanding in dealing with animals are reflected not only in the narrative itself but also in the unusually attractive photographic illustrations accompanying the text. The book is quite superior to the general run of animal stories.

THE OTTER BOOK.

By Phyllis Kelway. Wm. Collins Sons and Co., London and Glasgow. 4s. 144 pp. 1944.

Phyllis Kelway writes truly delightful descriptions of nature and vivid, lively accounts of animal ways. This story of the tame otter and its simple yet exciting adventures will be sure to keep its readers engrossed. The narrative device of allowing the otter to talk is a dangerous one, but the author has kept skilfully within bounds. The otter remains an otter, a mammal of distinct intelligence comparable to that of a dog, if of its own peculiar turn. The book is beautifully illustrated with pen and ink drawings and with eighteen photographs that must be ranked among the finest animal pictures ever taken.

ANIMAL EMBRYOLOGY

THE INDIVIDUAL IN SIMPLER FORMS. *University of Oregon Monographs, Studies in Psychology Number 2.*

By Arthur Russell Moore. University of Oregon Press, Eugene. $1.25 (paper). x + 143 pp.; 13 plates. 1945.

In considering the nature of individuality in embryogenesis and in animal behavior, the problem of the relation of the parts to the whole is ever present. According to the author, certain misconceptions of the nature of the problem have developed because of the emphasis ordinarily put upon the materiality of the parts. For instance, he thinks that the application of analytical methods in physiology and in biochemistry does not, in a comprehensive way, lead to an understanding of some of the most characteristic manifestations of individuality. It is true that in the field of animal behavior one meets types of conduct which could not have been predicted from the knowledge of the physical and chemical nature of the constituent elements of the organism. The cells which make up the body of a worm are not greatly different from those of the warm-blooded animal; yet there is a wide difference in the degree of complexity of conduct in the two forms. Something more, the author feels, is required to differentiate one organism from another.

This monograph, therefore, is an attempt to show, by illustrative examples, that the emergence of new patterns of form and of behavior is made possible by a third factor, the spatial arrangement of the morphological elements. In other words, one must consider not only the physics and chemistry of the organism, but the geometry as well. "This quality of the ordered arrangement of parts makes possible the development of organs, the specialization of function in differentiated tissues, head-tail polarity, and head dominance. The parts being given, the geometrical ordering of them gives coherence and character (that is to say, integration) to the embryonic development and to the reactions of the organism."

The problem of the meaning of ordered arrangement in giving coherence to the individual is dealt with, in terms of increasing complexity, in Chapters I to VI. After a consideration of the evanescent structure of the naked protoplast, there follows an analysis of the development of the free-swimming embryo in terms of structure and positional ordering of cells. The next step in progression deals with the spatial relations of the central nervous system as necessary factors in integration. Chapter VII is given to a consideration of modifiability of function by various chemical substances having particular qualities, with especial attention to the action of those which are the product of the animal's own metabolism. The final chapter deals with memory as a physiological concept.

The illustrative material is for the most part taken from work done in the author's laboratory (much of the experimental work having been carried out at the Hopkins Marine Station, Pacific Grove). As a whole the material is rather clearly and interestingly presented. While many of the issues are controversial, the viewpoint as set forth by the author is an interesting approach (not altogether new) to the larger, and certainly important problem of organization in form and movement.

MARY E. RAWLES

ANIMAL MORPHOLOGY

A HISTORY OF COMPARATIVE ANATOMY *From Aristotle to the Eighteenth Century.* *By F. J. Cole. Macmillan and Company, London.* $7.00. viii + 524 pp. 1944.

Everyone concerned with the structure of animals vertebrate or invertebrate, or with the processes by which science advances will be grateful to the author of this handy volume. It is well to remember at the outset that the period covered by the book ends before the rise of comparative anatomy in the modern sense. Within this limitation Cole has presented a storehouse of information based on a first-hand study of the old masters of anatomy. Included are some 200 figures, many of them full page plates, reproduced from both major and minor anatomical classics. Many of them are inaccessible outside the vaults of a few large libraries, so that it seems a pity that they were printed on such abominable paper.

The general plan of the book centers around the work of twenty-three anatomists arranged in chronological order, although there are final chapters on academies and museums and a brief conclusion. Greek anatomy is represented by Aristotle and Galen. Cole's treatment of Aristotle—presenting selections rather than a a survey of his work—employs the admirable method of following certain problems formulated by the great Stagirite, through many vicissitudes down into comparatively modern times. His treatment of Galen differs from that of some historians who appear afraid lest Galen corrupt their readers. Along with Galen's errors, Cole boldly depicts his many penetrating and accurate discoveries and points out that his work on the nervous system was not superseded until the time of Bell and Magendie.

The anatomists of the early sixteenth century are treated rather briefly, possibly because numerous books and figures are already available for da Vinci, Vesalius, and to a certain extent even for Belon. The main portion of the book deals with the work of investigators of the latter part of the sixteenth century beginning with Coiter, Ruini, Fabricius, and Casserius, and continues down to the end of the seventeenth century. Here the text is ample and the figures are numerous. One cannot help but mention three plates of exceptional beauty from Coiter including his famous figure of the skeleton of the Capuchin monkey, or the all but incredible vascular injections of Ruysch (who even injected the bladders used for sealing museum jars), or Nicholl's figure of the gynandromorph lobster—but of this there would be no end. Those familiar only with

Willis' work on the brain will be surprised by the extent of his anatomical investigations on the oyster, crayfish, and earthworm, then little known "Brutes." Malpighi is extensively though not exclusively represented by his work on the silkworm, which is compared with similar work by Pasteur. The Dutch school of anatomy is covered very fully, especially the work of Swammerdam.

Like many an anatomist, Cole is not averse to a good story, and he proves himself in the great tradition in this respect by leading off with what is perhaps the oldest of such tales—the story about why Democritus, having retreated to the amenities of the local cemetery to pursue his studies in comparative anatomy, was visited by the Father of Medicine. The weakest part of Cole's work undoubtedly lies in his conclusions (fortunately brief), which are set in a hopelessly outmoded late nineteenth century framework. As a whole, the book is a noteworthy addition to the armamentarium of any anatomist who aims to be more than a technician. There is an extensive bibliography of source material.

GAIRDNER MOMENT

ANIMAL PHYSIOLOGY

FRANCOIS MAGENDIE. *Pioneer in Experimental Physiology and Scientific Medicine in XIX Century France. By J. M. D. Olmsted. Schuman's, New York.* $5.00. xvi + 290 pp. 1945.

In the history of science it is most disadvantageous for a man's reputation either to be the teacher or to be the pupil of a very great man. In such a case all that is remembered of him is in general that he was "the teacher of . . ." or "the pupil of . . ." This effect is very strikingly observed in the reputations of Claude Bernard's teacher, Francois Magendie, and of his pupil, Paul Bert. J. M. D. Olmstedt, professor of physiology at the University of California, to whom we owe already a most excellent biography of Claude Bernard, shows in this new biography that Magendie is very much worth remembering for his own sake. He was actually the initiator of experimental pharmacology and of experimental physiology that led beyond the most brilliant accomplishments of the great contemporary French clinicians. Olmstedt gives us a full view of Magendie's many-sided work and life, of his strange education and character, and he devotes considerable space to the famous Bell-Magendie controversy.

One often feels inclined to complain that the history of science is written to so great an extent in the form of biographies. But if all biographies were to be such as this, there would be no reason to complain. Great readability is obtained, though all cheap effects are avoided. Personal and scientific details, general historical background, and specific medical data are all well balanced in their proportions. It is one of the very few books one enjoys reviewing.

ERWIN H. ACKERKNECHT

THE HUMAN BODY. *Fourth Edition. By Logan Clendening. Alfred A. Knopf, New York.* $4.00. xv + 443 pp.; 20 plates. 1945.

It is natural to wish to have a special word of kindness for this final edition of the late Kansas doctor's book on the fundamentals of human anatomy, physiology, and pathology. This is unfortunately hardly possible. Changes from the previous edition have been very minor ones. Most of the old errors remain uncorrected, and they are not few in number. However, *The Human Body* is still one of the outstanding books of its kind, and if the virtues of the final edition are those of its predecessors, they are not slight. Few biological books have been written with more humor or more zest. Few have less of the musty flavor of scholastic learning, or so sharply prick and puncture the foibles and misconceptions of laymen and doctors alike. Few are so likely to encourage in their readers as lasting a taste for knowledge of their physical selves, or as eager a curiosity to pursue further the study of life.

LECTURES ON THE KIDNEY. *Porter Lectures—Series IX; The William Henry Welch Lectures. By Homer W. Smith. University Extension Division, University of Kansas, Lawrence, Kansas.* $1.00. 134 pp. 1943.

Three of the lectures which comprise this book were delivered at the University of Kansas School of Medicine in 1939 as the Porter Lectures. The content of these lectures is indicated by their titles: The Evolution of the Kidney; Newer Methods of Study of Renal Function in Man; and The Renal Blood Flow in Normal Subjects. The remaining two chapters of the book are entitled: Renal Physiology Between Two Wars; and The Application of Saturation Methods to the Study of Glomerular and Tubular Function in the Human Kidney. These two lectures, the William Henry Welch Lectures, were delivered at the Mount Sinai Hospital, New York City, in 1943. The summary of the probable course of development of the kidney elements as found in mammals, reptiles, teleost, and elasmobranch fishes is very interesting and convincing. In section four the author relates how the peculiarities of kidney structure and function in various species have been used to clarify the mechanism of function of the mammalian kidney. This account of the progress made since 1917 is very interesting and instructive. The subject matter of sections two, three, and five has become well known to physiologists, and much of the material has been incorporated in recently

revised textbooks. These three lectures, however, present the information obtained in recent studies of the kidney much more clearly and fully than do textbooks and numerous recent reviews. The last of the sections deals principally with the problems presented by possible intermittency of glomerular activity and tubular perfusion, which may occur in the human kidney. It is a detailed account of modern methods of studying human kidney function and the information that has been obtained by means of these techniques. This small volume of lectures is recommended to those interested in the physiology of the kidney and the cardiovascular system.

CHANDLER McC. BROOKS

RECENT ADVANCES IN ENDOCRINOLOGY.

By A. T. Cameron. The Blakiston Company, Philadelphia. $5.00. vii + 415 pp. 1945.

The preface of the first (1933) edition of Cameron's *Recent Advances* presents endocrinology as a biochemical subject to the degree that "the precise truth of its teachings depends ultimately upon the isolation of the different endocrine principles in pure crystalline form, so that their physiological and pharmacological properties may be ascertained accurately ... the final problem, the elucidation of the precise mechanism of the actions of these principles will require profound and prolonged biochemical and physiological study." It is further stated that clinical medicine shares with physiology, biology, anatomy, and pathology the task of indicating methods of test whereby these ends may be achieved. In the present edition, it is stated that "at the suggestion of the publishers, the clinical aspects of endocrinology have received more attention ... and the volume has had to be almost completely rewritten." The result is a volume that diverges considerably from its title, and also from the council of perfection originally set for it; it is, and can be recommended as, a textbook of clinical endocrinology.

A real attempt appears to have been made to digest the recent literature, as for example, in the sections on hyperthyroidism, cardiotoxic goiter, panhypopituitarism, and adrenocortical syndromes. The section on chemical transmission of nerve impulses, however, has been carried over from a previous volume and ignores recent and exciting developments in this subject. Alloxan diabetes managed to squeeze into this volume, but much work evidently arrived too late for inclusion.

The custom of treating the endocrine system by "glands" dies hard; the author's present section on "Integration of the action of hormones associated with reproduction" and the chapter on "Cushing's Disease and the Adrenocortical Syndromes" represent successful attempts to escape the glandular incubus.

The second "fundamental concept" of the author, that a gland may produce too much or too little of a specific compound, but that it does not produce abnormal compounds, has served a useful turn, especially in reference to abnormal thyroid states; but in view of the work of Rhoades et al. some modification of this dictum may be necessary. Steroid compounds (hormones?) arise in adrenal dyscrasias and in cancer which are apparently absent in normal individuals.

H. R. CATCHPOLE

ANIMAL NUTRITION

NUTRITION WITH SENSE.

By Eleanora Sense. M. Barrows and Company, New York. $2.00. 222 pp. 1944.

A complete knowledge of nutrition includes not only an understanding of the daily food requirements for the human body, but also a comprehension of the need for careful selection and preparation of the available foods, as well as their nutritional values, in fulfilling those requirements. The present volume is designed to furnish just such a complete and up-to-date knowledge of nutrition for the average home-maker.

The basic foods (proteins, carbohydrates, fats, vitamins, and minerals) are in turn discussed in the light of body needs, food sources, and a daily nutrition menu pattern. The importance of obtaining sufficient quantities of the various food elements in the proper combinations to maintain vigorous growth in children and general good health in adults is stressed throughout the text. Emphasis is also placed on methods of preparing foods, not only for conserving the vital food values, but also for combining them into attractive and appetizing meals. Excellent chapters are included to cover such pertinent topics as nutrition in relation to soil and methods of agriculture, economical food marketing and buying, as well as rules for cooking. The latter half of the volume is devoted to carefully prepared menus and recipes. A table of food values, a subject index, and an index for menus and recipes are appended.

The work will undoubtedly find popular favor on the high school or college home economics reference shelf, as well as in the home kitchen library.

B. AUBREY SCHNEIDER

CELL PHYSIOLOGY

THE PERMEABILITY OF LIVING CELLS. *Protoplasma-Monographien Volume 19.*

By S. C. Brooks and Matilda Moldenhauer Brooks. (Gebrüder Borntträger, Berlin-Zehlendorf); J. W. Edwards, Ann Arbor, Michigan. $5.00. xviii + 395 + 14 pp. (1941); 1944.

The integrity of its plasma membrane is fundamental, not only to the physiological activity, but to the very existence of the cell, and consequently any experiments or theories that lead to information regarding the intimate nature of this membrane are of great interest to all workers in the biological field. The existence of the membrane is primarily inferred from the differential rates of penetration of substances into the cell, and this characteristic, the permeability of the cell membrane, still remains the most important index to the structure of the plasma membrane, in spite of the contributions of x-ray diffraction, surface tension, electric impedance, and electron microscope studies. The appearance of a new monograph on the permeability of cell membranes by such well known authorities as S. C. Brooks and M. M. Brooks will therefore be viewed as a welcome contribution to this involved subject.

The book was completed in 1939 and unfortunately, as it turned out, was sent to Germany for publication, so that the war effectively prevented its distribution outside Germany. The present edition is an American (J. W. Edwards) lithoprinted reproduction of the German edition and, as such, exhibits numerous technical defects attributable to the difficulties of war-time printing in Germany, e.g., misprints are disturbingly frequent and, every few lines, there is an instance of defective type; on page 31 there is a reference to a non-existent table, and so on. The book thus appears some six years later than the authors intended, and in a cruelly mutilated form.

In the preface, the authors tell us that they had started out ambitiously to cover the whole of the factors involved in the study of permeability, but that only a fraction of what they desired to present had finally come out of their past hopes. They conclude with the statement: "And so we have finally attempted to crystallize into a critique a few of the factors which we have deemed important in connection with the present-time experimental inquiry into Permeability." In reading the monograph, it will be necessary to bear this concluding statement well in mind, for otherwise one may be disappointed, since very many aspects of permeability have either been summarily dealt with or ignored altogether.

The book may be divided into three main parts, consisting of discussions of osmotic phenomena in cells and tissues, of the permeability of cells and tissues to strong electrolytes, and of the permeability to dyestuffs. Chapters on the permeability to non-electrolytes, weak electrolytes, and gases are included in the book, but it is clear from their perfunctory nature that the authors had not their hearts in this part of their enterprise.

To that part of the book dealing with osmotic effects, the reviewer is happy to give warm praise; it is an excellent review of the literature on water exchanges in a wide variety of cells and tissues, viewed both from the (static) equilibrium and the (kinetic) permeability aspects, and the problem of "bound" water is handled with astute critical ability. A minor criticism is that, in dealing with the osmotic "anomalies" of the erythrocyte, the authors, while adducing a lot of material of questionable scientific value, ignore more modern chemical studies bearing on the problem; moreover, their tentative explanation of the anomalies on the basis of electro-osmosis, in consequence of the thermodynamic potential across the membrane, appears to the reviewer very questionable.

The chapter on strong electrolytes is obviously a labor of love; it concerns a branch of the permeability problem with which the authors have been intimately connected for many years and one on which they can speak with authority, yet to the reviewer this chapter is less satisfactory. There are references to the Brooks' theory of accumulation, but this is developed and presented in so obscure a manner that the reader is left with only a vague idea of its essentials and its relation to other theories and the established facts. The permeability of cells to strong electrolytes is very frequently bound up with secretory processes, i.e., the driving force impelling the electrolyte to move is not necessarily the difference in observed activity between cell and surrounding medium. The simplest and most illuminating studies of ionic permeability must therefore be those concerned with ions not greatly, if at all, connected with secretory processes. The erythrocyte probably approaches such a system better than any other cell, but the authors have paid it very scant attention. The result is that ionic permeability, as such, and ionic accumulation are indissolubly mixed in their treatment, and the issue is confusing, especially to the uninitiated reader.

The section dealing with dyes is thorough, and the pages are crowded with references; to those biological workers whose interest in permeability is mainly due to the practical problem of vital staining, it should be invaluable. In the opening paragraph, the authors criticize very forcibly the limitations of dye techniques in permeability studies and seem to be under no illusions as to the illogicality of devoting special chapters to work involving these substances, and it seems unfortunate that they bowed to convention to the extent of devoting some 80 pages to this topic. It is unfortunate because this procedure has led them to discuss the various theories of membrane structure and permeability in an atmosphere tainted by the unreliable nature of much of the experimental work; it would surely have been more satisfactory to have discussed these theories in the light of the more recent quantitative studies on non-electrolytes. As it is, the chapter on non-electrolytes consists of a perfunctory 20 pages and, as if it had not been robbed enough, the important work of Collander and Bärlund on non-electrolytes has been included in the chapter on strong electrolytes.

The book, as a whole, shows unmistakeably that the

authors have been at great pains to collect and sift the literature dealing with those aspects of permeability that have interested them most, and they have, moreover, shown no hesitation in frankly and fairly criticizing work which they considered bad. The defects in the book, as the reviewer sees them, lie chiefly in the mode of presentation—the thread of an argument is broken by the indiscriminate lumping in of facts which might have been postponed to a more appropriate place; the important difference between equilibrium conditions and permeability processes, although doubtless clearly appreciated by the authors, are obscured by the failure to discuss these in separate chapters; the problem of secretion should have been kept separate from that of simple permeability; the material dealing with the penetration of non-electrolytes should have been kept separate from that dealing with the effects of non-electrolytes on permeability; and so on. In consequence, the book is of value to the expert interested in one or all of the three aspects of permeability on which the book concentrates. To the teacher of biology or the graduate student, searching for a guide through the complex maze of permeability literature, it offers but meagre aid.

HUGH DAVSON

BIOCHEMISTRY

THE SPECIFICITY OF SEROLOGICAL REACTIONS. *Revised Edition.*

By Karl Landsteiner. With a Chapter on Molecular Structure and Intermolecular Forces by Linus Pauling. Harvard University Press, Cambridge, Mass. $5.00. xiv + 310 pp. 1945.

The German and the first English editions of this book received brief mention in these columns, whereas the present edition has been considered by the editors as warranting a more detailed review. This does not reflect any basic changes in the book, which has been considered a classic since its first appearance; rather, it illustrates the enhanced interest in serological phenomena among biologists. Biology is dominated by problems of specificity; the two main approaches now available to interpret specificity on a chemical or physico-chemical basis are those of enzymology and serology. The last fifty years have seen serology grow out of its bacteriological childhood into a full-fledged branch of the biological sciences. The interest of geneticists, in particular, has recently been heightened by the work of Irwin and his group, suggesting a direct relationship between genes and cell antigens. The hypothesis that antigens may represent complete or partial replicas of genes has directed attention to the possibility of modifying the genes by means of antibodies against cell antigens; reported successes along these lines by Guyer and Smith on lens defects in rabbits, and more recently by Emerson on *Neurospora*, await more extensive confirmation.

This second English edition of Landsteiner's book appears after the death of the author, who has more than any other worker been responsible for the development of its subject matter. It is interesting to recall that, while Landsteiner's discovery of the blood groups in 1900 started serology on its way toward wider biological applications, his work on artificially synthesized antigens, begun in 1914, gave it a solid chemical foundation. In the present edition, most chapters are amplified and two new chapters are added *ex-novo*. The result is a most comprehensive discussion of present-day ideas of serological specificity and of their historical development.

The serological specificity depends on and closely reflects the chemical structure of the antigens is now an established fact. Specificity can be traced to certain determinant groups of various degrees of complexity, which the antibody supposedly "fits" as a key fits a lock. All serological reactions are considered as due to specific attraction between antigen and antibody, with the limitation that antibody "specificity" is directed toward the determinant groups rather than toward the antigen as a whole. Cross-reactions may arise either from the sharing of common determinants by two antigens, or by structural similarity of determinants, in which case specificity generally takes the form of stronger affinity of the antibody for the homologous antigen.

The physical basis of this affinity, and its relation to the mode of antibody production, have recently received an appealing interpretation by Pauling. Antibody formation is considered as being due to the folding of the plastic polypeptide chains of the globulin molecule in such a way as to fit the mold provided by the active groups of the antigen surface. The resulting complementariness of surface patterns then permits a relatively large portion of the surface of antigen and antibody to be brought together at very close range. Specific attraction reflects the specificity of the patterns, and results from the summation of numerous aspecific forces between atoms and groups of atoms thus brought into play. Biologists will welcome the inclusion in this edition of a chapter by Linus Pauling, in which he summarizes the nature of intermolecular forces and briefly indicates how they may be supposed to act in antigen-antibody reactions. This summary, very clear and to the point, will help those who do not feel equipped for the study of more complete treatments of this topic.

For the biologist, some of Landsteiner's conclusions regarding natural antigens and species specificity are worth analyzing more closely. In the words of the author, "... there exist two systems of species specificity in the animal kingdom, the specificity of proteins and that of cell haptens. The proteins, it would seem, undergo gradual variation in the course of evolution, while haptens are subject to sudden changes

not linked by intermediary stages." Typical of cell haptens are, for example, the blood group substances and in general the red blood cell antigens, and the Forssman antigen. It is to be expected that the relatively small, highly active groups responsible for cell hapten specificity would show serological variation in discrete, well defined steps, since allelic differences in one or sometimes a few genes may deeply affect their relatively simple structure. Cross-reactions between physiologically or systematically unrelated cells are also to be expected, since antibodies against small, very active determinant groups often give marked cross-reactions with chemically related haptens, such as may be present in very different cells.

The supposedly "gradual" systematic variation of proteins might appear more difficult to reconcile with our current ideas of evolution by accumulation of discrete genetic changes. On the one hand, however, the work of Cumley and Irwin on serum proteins in species hybrids of doves seems to indicate that, as in the case of cell antigens, species differences in protein antigens may be reducible to discretely segregating units. On the other hand, protein variation within a species and between species may actually occur discontinuously, by discrete changes, even when serological tests do not indicate it. Serological analysis, though a very refined tool, lays no claim to finality. For proteins, in particular, we generally consider the antigenic determinants as large areas of the molecular surface, whose overall configuration, rather than one or a few strongly active groups, is responsible for serological specificity. Changes in one or two groups may affect only slightly the specific reactivity of the overall pattern, as indicated also by experiments with polypeptides. If we imagine that a large number of genes are involved, through a number of enzyme systems, in protein synthesis, it is not strange that *serologically detectable differences* only appear after a relatively large number of genetic changes have taken place. It should be added that if sudden major changes in protein structure occur, they may have a lower survival value due to the delicate functional test to which most proteins are subject in the organism. This need not be so for some cell haptens, as for instance in the case of the blood group substances, which as yet have not been found to perform any important function.

The serological specificity of physiologically different proteins, like hemoglobin, serum albumin, and keratin, is simpler to interpret, because of known differences in their chemical composition. It is, however, intriguing that detectable antibody formation rarely follows the injection of any protein from the same species or individual. One does not feel entirely satisfied with the explanation, very tentatively mentioned by Landsteiner, that in such cases antibodies are actually formed and rapidly fixed by the proteins of the organism itself. On the other hand, it is difficult to see how the antibody-forming cells could recognize every homologous protein antigen, even though each cell has a complete set of genes similar to those which directed the synthesis of the antigen.

A field which seems to have been the object of surprisingly little investigation is that of developmental serology. It seems to the reviewer that various lines of evidence (recapitulation, transplantation, virus research), alike suggesting the possibility that tissue and protein specificity in embryonal life may be different than in the adult, should encourage more work in this field.

It is characteristic of this book that theoretical questions, such as those we have discussed here, are never dogmatically answered nor receive more emphasis than their experimental basis justifies. Cautious warnings as to contradictory findings and alternative interpretations are rather the rule. The enormous wealth of material often gives the book the apparent character of a bibliographical review, in which, however, every result is evaluated, interpreted, and coordinated in a broader picture. This very wealth of material makes the book difficult reading; observations pertinent to one topic are often scattered in different sections, important generalizations buried in the middle of a paragraph. Nor is the reader helped by the rather peculiar system of bibliographic references, or by the extensive use of chemical terminology with little assistance from structural formulas.

The character of the book seems to reflect the character of Karl Landsteiner's life work, probably more preoccupied with an attack upon basic problems in their fundamental aspects than with the attainment of a final, self-contained picture of this or that special problem. It is significant that throughout both this book and the monumental work of its author one can trace, along with the preoccupation with the chemical basis of serological phenomena, a continuous awareness of their biological implications.

S. E. Luria

Colloid Chemistry *Theoretical and Applied. Volume V. Theory and Methods; Biology and Medicine. By Selected International Contributors, collected and edited by Jerome Alexander. Reinhold Publishing Corporation, New York.* $20.00. vi + 1256 pp. 1944.

This weighty volume contains no less than sixty contributions by outstanding authorities on various subjects closely or distantly related to colloid chemistry. It is a volume that will be of special interest and usefulness to biologists, since, in addition to twenty-five contributions dealing with theory and methods, there are thirty-five directly in the field of biology and medicine. Such a wealth of material forbids even a mention by title of all the individual contributions. A

few general comments and an arbitrary selection of a few papers for special mention must suffice.

In general, the contributions are of a type more interesting and more valuable than those, for example, in the *Annual Reviews*. Subjects are discussed critically and in a rounded way, the aim not being to cite all recent papers within a limited field but to treat each subject in such a way that a reader of even limited special background might grasp the discussion. One is consequently amply repaid for the slow going throughout these hundreds of closely printed pages. The presence of numerous diagrams and half-tones is helpful, although many of the half-tones are poorly reproduced. Each article has its separate bibliography, some of them listing 100–250 references. There are general author and subject indexes.

In the first section, that dealing with theory and methods, many of the papers will prove over-technical for most biologists. Electron and x-ray diffraction, molecular films, Liesegang rings, polymerization, the electron microscope, cyclotron, betatron, and high speed centrifugation are only a few of the subjects discussed.

Section II, on Biology and Medicine, starts with a fundamental discussion of protein structure by Astbury. Rothemund's review of photosynthesis is helpful in bringing one up to date. Wanda K. Farr makes an exhaustive review of work on the plant cell wall, but helps to confuse terminology by steadfastly calling it the plant cell membrane. There is a very rewarding discussion of enzymes and vitamin action by Axelrod and Elvehjem. Minerals and vitamins in applied nutrition (Booker) is also a mine of condensed information. The discussion of the mechanism of hormone action attempts to look ahead; Kamm and Kitchen, however, have endeavored to adopt a narrow definition of "hormone" that seems so untenable that they were not even able to maintain it consistently themselves. Wald on visual pigments and Hudson Hoagland on chemical pacemakers are both extremely interesting, the latter work on the identification of enzyme systems that are limiting factors or "bottlenecks" in metabolic processes promising great advances in the future. Lauffer and Stanley have contributed a further review on purified viruses, and Jack Schultz a discussion of the gene as a chemical unit. The latter paper will probably be of chief interest to advanced geneticists. Menkin's work on leukotaxine makes an interesting review. Boyd contributes an excellent review of antigen-antibody reactions. Cannon has made a concise summary of homeostasis, while Leo Loeb's review of the causes and nature of cancer is masterly and deserves much fuller comment than it can receive here. A discussion of gerontology (A. J. Carlson) and two interesting contributions on colloidal relations in neuropsychiatric disorders (Spiegel and Spiegel-Adolf; Ludlum) must also be mentioned.

The final contribution deserves a separate word. It is from the long-silent C. R. Plunkett, and is on "The Primary Physicochemical Basis of Organic Evolution." The wealth of original thought, synthesis of ideas, and fruitful hypothesis here can only be appreciated after careful study. To every student of evolutionary process and gene action it is recommended, in the hope that it will prove to be a mental catalyst of superior activity. For this one contribution alone, to say nothing of the great value of the 59 others, one ought to be willing to meet the rather high cost of the volume.

BENTLEY GLASS

POLYMER BULLETIN. *Published with the cooperation of the Bureau of High Polymer Research, Polytechnic Institute of Brooklyn, New York—Herman Mark, Director. Bimonthly. Volume I, Number 1, April 1945.*

Editor, Paul M. Doty. Interscience Publishers, New York. $2.40 a year; foreign, including Canada, $2.90 a year.

"To provide a medium for the dissemination of information on trends, developments and research in the rapidly growing field of high polymers" . . . "summarizing the seminars and symposia held at the Institute" . . . "news items in this field" . . . "contributed papers."

REPORTS OF THE BIOCHEMICAL RESEARCH FOUNDATION OF THE FRANKLIN INSTITUTE, *Volume VII, 1942—1943.*

Collected Papers. Biochemical Research Foundation of the Franklin Institute, Newark, Delaware. $3.50. (1944).

MICROBIOLOGY

CONFERENCE ON GENE ACTION IN MICROORGANISMS. *Special Number, Annals of the Missouri Botanical Garden, Volume XXXII, Number 2, April, 1945.*

The Missouri Botanical Garden, St. Louis. $5.00 (paper). Pp. 107–263. 1945.

This special number presents the papers and discussion of the Conference on "Gene Action in Micro-organisms" held February 2–3, 1945, in St. Louis. Microbiologists, geneticists, general physiologists, and biochemists will find it exceptionally worth their attention. Papers by Demerec, Hollaender, Gowen, Delbrück, and Luria deal with mutations of penicillium, bacteria, and viruses affecting resistance and virulence. Another group of papers deals with gene action. To this group Lindegren and Spiegelman, Tatum and

Beadle, Sonneborn, Greenstein and Chalkley, and Sterling Emerson have contributed. The study of enzymatic adaptation in yeasts, of biochemical mutations in Neurospora, of the killer gene and kappa interaction in Paramecium, and of the differences in effect of ribose and desoxyribose nucleic acids on rates of methylene blue reduction by aqueous extracts of liver have added much to present knowledge of the mechanism of gene action and have added the plasmagene to the concepts that must definitely be reckoned with. Emerson presents a graphic and intelligible theory of gene-enzyme-substrate specificity as related to gene reproduction. These fertile discussions should lead to much productive research.

THE BACTERIAL CELL *In Its Relation to Problems of Virulence, Immunity and Chemotherapy. Harvard University Monograph in Medicine and Public Health Number 6.*

By Rene J. Dubos, with an addendum by C. F. Robinow. Harvard University Press, Cambridge, Mass. $5.00. xix + 460 pp. 1945.

No phase of bacteriology is more fundamental, nor has interested more investigators in the field, than the bacterial cell itself. The literature which has accumulated is enormous, but there has been nothing available having either the scope or the excellence of this book. It deals with cellular morphology, physiology, and chemistry, with stability and variation, and with the phenomena of virulence, immunity, and antibacterial action in their relation to the nature and organization of cell components. Particularly emphasized are the properties of specificity and variability of the bacterial cell.

The various subjects are developed with discerning clarity. A tremendous amount of diverse material is integrated simply into a concise, intelligible, and thoroughly readable whole—no mean achievement in view of the elaborate vocabularies of the highly specialized sciences concerned. Apparent inconsistencies and inadequacies of present day knowledge are pointed out, as well as conflicting opinions. Lines of thought for future research are suggested. This outstanding book should be read by every bacteriologist, by those interested in the mechanisms of infection and resistance, and by biochemists.

Following the text is an added paper on nuclear apparatus by C. F. Robinow of the Strangeways Laboratory in England. His findings concerning the bacterial equivalent of chromosomes, the nuclear structure of spores, and cell division in the genus *Bacillus* are summarized. The book is attractively illustrated and well indexed; it has a lengthy bibliography.

HARRIETTE VERA

MICROBIAL ANTAGONISMS AND ANTIBIOTIC SUBSTANCES.

By Selman A. Waksman. The Commonwealth Fund, New York. $3.75. ix + 350 pp. 1945.

The comparatively recent and almost explosive development of the field of knowledge of antibiotic action makes this volume particularly useful as a reference for those who have not been especially involved in the investigations. Written from the point of view of the soil microbiologist, the introductory chapters are devoted to that subject, and emphasis, throughout the book, is directed toward it. The reporting of specific contributions, although the results have frequently been contradictory, is detailed and thorough; the bibliography is extensive. The well-planned material on methods and the frequent tables and charts should be exceedingly useful to teachers and to students interested in antibiotics.

The scope of the book is broad. Chapters on the chemistry of antibiotic substances and the nature of antibiotic action are comprehensive, and the limitations of present day information are indicated. Both plant and animal diseases are considered, and the interrelationships of organisms in their habitats are discussed. Considerable material is presented about penicillin and other substances which have been used clinically, but, on the whole, public health aspects are not emphasized. In the concluding chapter problems in need of clarification are brought out, and possibilities for future research are suggested.

A number of photographs are included, and there is an index of microorganisms as well as a more general subject index.

HARRIETTE VERA

BACTERIOLOGY AND ALLIED SUBJECTS.

By Louis Gershenfeld. Mack Publishing Company, Easton, Pennsylvania. $6.00. xxiii + 561 pp. 1945.

This volume, which is an outgrowth of the author's earlier *Bacteriology and Sanitary Science*, is written primarily for pharmacy students and should serve that purpose adequately. Moreover, it ought to be useful as a text or as a reference for students in other fields related to bacteriology.

A large amount of practical information is included between the covers of this text, and the material selected differs somewhat from that of other similar works. For students expecting to specialize in bacteriology, the treatment of bacterial physiology seems to be rather limited. Such "allied subjects" as insect control, insecticides, fumigation, and biological products, which are often missing or sketchily discussed in most texts, are treated in some detail. Appropriately enough, the section on disinfection is extensive. There is comparatively little of the public health aspect of bac-

teriology. Illustrations are few and uninteresting. The index is satisfactory.

HARRIETTE VERA

HEALTH AND DISEASE

SAFE AND HEALTHY LIVING. *I. Spick and Span. II. The Health Parade. III. Growing Big and Strong. IV. Safety Every Day. V. Doing Your Best for Health. VI. Building Good Health. VII. Helping the Body in Its Work. VIII. The Healthy Home and Community. New Edition.*

By J. Mace Andress, I. H. Goldberger, Marguerite P. Dolch, and Grace T. Hallock. Ginn and Company, Boston. I, 84 cents; II, 88 cents; III, 92 cents; IV, 92 cents; V, 96 cents; VI, $1.00; VII, $1.04; VIII, $1.08. I, viii + 152 pp.; II, viii + 184 pp.; III, xii + 252 pp.; IV, x + 258 pp.; V, viii + 298 pp.; VI, viii + 298 pp.; VII, viii + 314 pp.; VIII, viii + 339 pp. 1945.

Attractive, well illustrated, carefully graded books like these should prove to be well liked. To an adult biologist looking back, there may seem to be an undue amount of repetition of subject matter year by year. To the average child this is probably not unduly noticeable, and, of course, a certain amount of review is required for retention. At any rate, my own children like them a great deal.

The books include units dealing in the early grades chiefly with good health habits, and introduce more and more human anatomy and physiology, bacteriology and sanitation as the child's education advances. In Book VIII, for example, there are 12 units, dealing successively with: your health; warmth, light, and air at home; community health; safe water and milk; the safeguarding and preparation of food; community and home cleanliness; buying food and planning and serving meals; communicable disease; insects that transmit disease; taking care of the sick and injured; home safety and first aid; the health of the mind. The treatment of alcohol and tobacco is sane and scientifically sound, avoiding on the one hand the missionary fervor of the prohibitionist and on the other the blindness of those who deny that these drugs create any problem whatever. Books IV–VIII contain lists of suggested books to read, all books have vocabulary sections, and all but I and II have indexes. Tests, exercises, and suggested activities are included; but there is little use of experiment or demonstration, even in the course for the eighth grade. A good teacher, of course, will be able to supply these; but it is disappointing not to find them here, along with so much else that is excellent and thoughtfully planned.

Beginning with Book V, the history of science and medicine as well as that of pertinent social change is successfully interwoven with the discussions of health subjects. Of all the many modern and valuable features of these texts, perhaps this one is most laudable. There is a distinct break with the hard and bitter recollections of the "hygiene" of our own youth as a set of tedious rules.

BENTLEY GLASS

THE MARCH OF MEDICINE. *Number IX of The New York Academy of Medicine Lectures to the Laity, 1944. By Edward A. Strecker, Charles Glen King, Colin M. MacLeod, Reginald Fitz, Sir Gerald Campbell, and Lt. Col. Thomas T. Mackie. Columbia University Press, New York.* $1.75. xiv + 121 pp. 1945.

It may be unfair to compare this series of lectures to laymen, given under the auspices of the New York Academy of Medicine in 1944, with the writing on medical topics for the laymen to be found in popular magazines and journals during the same year, as collected, for example, in the *Science Yearbook of 1945*, reviewed elsewhere in this issue. Yet it is obvious that the eminent lectures of this series seem to have had less to say, and have said it more pontifically, than those who undertook to write in simple narrative style for the readers of "Time" or "Collier's." Is it perhaps no more than the effect of a stiff shirt front and a wing collar confronted by orderly rows of faces in a select audience? At all events, once more it has been demonstrated that most lectures make mediocre reading. Not to be hypercritical, however, there is a good deal in these few pages that was worth saying and was indeed well said. For example, to quote E. A. Strecker on the theme of morale and propaganda:

"It must not be inferred that, in order to be honest, propaganda must confine itself to categorical statements of facts, coldly stated. The natural forte and appeal of propaganda is emotional. However, the emotions are not allergic to the truth, nor do they have an affinity for lies. If anything, they are more deeply moved by the truth. But even the truth must be emphatically, warmly, and stirringly stated."

Other lectures dealt with nutrition (King), chemotherapy (MacLeod), medicine in society (Fitz), the effects of science upon man (Campbell), and war and epidemics (Mackie). The historical account of chemotherapy is especially good.

BENTLEY GLASS

YOUNG MINDS WITH OLD BODIES.

By Melvin E. Page. Bruce Humphries, Boston; The Ryerson Press, Toronto. $2.50. 184 pp. 1944.

The author of this book claims to have discovered, as a result of a study of dental decay, the basic cause of all diseases. He has also found the method of remedying these basic defects and has published this book solely for the purpose of aiding those who may wish to be

aided, and in order that what he has discovered may not be lost.

The first sentence of chapter one is: "Why is it that modern man has a much shorter life span than any other animal?" This, of course, is not correct but it tells one all he needs to know about the book. A good example of the author's method of thinking and his scientific procedures is to be found on pages 26 and 27, where he gives a brief description of his technique of determining endocrine dysfunctions, sympathetic-parasympathetic imbalance, and general metabolism by comparing measurements of the circumferences of various portions of the arm and leg. The book does contain a few well-known truths and some correct statements of fact, but for the most part it is a dismal, uninteresting collection of misconceptions and absurdities. The reviewer does not wish to question the author's right to write a book nor does he wish to criticize the author's desire to aid his fellow men by attempting to cure disease, but such a book as this can be of no value to anyone. Any biologist who reads it will merely acquire a feeling of apprehension for those who need medical aid but who may receive treatment from the hands of unintelligent, uninformed practitioners.

CHANDLER MCC. BROOKS

YOUR HAIR AND ITS CARE.

By Oscar L. Levin and Howard T. Behrman. Emerson Books, New York. $2.00. 184 pp. 1945.

Although it is something of a shock to learn that the scalp and hair are considered as probably the dirtiest and most diseased portions of the human body, it is equally gratifying to learn that they are now receiving some of the personal and professional attention they deserve. In the present volume, two eminent dermatologists have presented, in clear, non-technical language, the latest pertinent information on the physiology, pathology, and care of the scalp and hair.

The point of emphasis running through the entire discourse is that if the scalp is kept clean, and if the general body health and constitution are kept at high levels of tone and function, there is no need to worry about premature baldness or gray hair, unless, of course, there is a hereditary tendency toward such characteristics. Much space is devoted to pointing out the dangers inherent in the misguided use of many of the highly advertised soaps, creams, oils, dyes, endocrine substances, and mechanical devices in attempting to beautify the hair. The beneficial effects of sunlight, x-rays, ultra-violet rays, and vitamins in preserving the hair and in maintaining general good health are emphasized. The latest methods in the treatment of a number of infectious scalp diseases are carefully outlined. A concluding chapter giving questions and answers relating to the care of the scalp and hair and the treatment of numerous scalp disorders encountered in everyday living summarizes the important points brought out in the text.

The work will be read and appreciated, not only by physicians, beauticians, and barbers, but by the general public as well. There is no index.

B. AUBREY SCHNEIDER

PROFITABLE AND NECESSARIE BOOKE OF OBSERVATIONS.

By William Clowes. Introductions by De Witt T. Starnes and Chauncey D. Leake. Scholars' Facsimiles and Reprints, New York. $6.00. XXVIII + 232 pp. 1945.

This facsimile reprint of a book of 1596 deals mainly with two problems still of central importance in war medicine: the treatment of gun-shot wounds and of venereal diseases. Having served with the British army and navy, Clowes had considerable experience in the matter. Though Clowes was no great inventor in the field of surgery, like Paré or Brunschwig, he was an honest craftsman and a good teacher. His numerous case histories, presented in a lusty Elizabethan English, make his book a valuable historical document beyond the narrower field of medical history. The reprint is accompanied by two excellent introductions by De Witt T. Starnes and Chauncey D. Leake. Attention should be drawn to Clowes' treatment of scurvy (p. 40 ff.), one of the earliest "vitamin" treatments on record.

ERWIN H. ACKERKNECHT

THE ETIOLOGY, DIAGNOSIS, AND TREATMENT OF AMEBIASIS.

By Charles Franklin Craig. The Williams & Wilkins Company, Baltimore. $4.50. viii + 332 pp. 1944.

In 1934 Colonel Craig published a monograph entitled *Amebiasis and Amebic Dysentery*. The present book is essentially a revision of that excellent publication. At the time of the first writing, there was considerable misunderstanding with regard to the symptoms produced by infection with *Endamoeba histolytica*. Although the term *amebiasis* includes amebic dysentery, as only a single manifestation of the disease, the medical profession was more familiar with the latter term. This being the case, Craig at that time included both of them in the title, in order to emphasize the close relationship between the two. It is interesting to note that now only the all-inclusive term, *amebiasis*, appears in the title. This may indicate that the author feels that the lesson taught by the original title has now been amply learned. If this is true, his excellent monograph is largely responsible for this progress.

Shortly before the appearance of the original monograph, the medical profession in this country had been

shocked into the realization that not only does amebiasis exist in the United States, but that severe epidemics of the disease can occur in as northern and modern a city as Chicago. With this sobering realization in mind, considerable attention is now being paid to the acquisition of this protozoan infection by service men stationed in tropical countries, and to the possibility of their intensifying this public health problem when they return to this country as carriers of the disease. It is therefore particularly important and timely that Colonel Craig should bring us up-to-date with the facts on amebiasis.

Much information has accumulated during the past ten years that has helped to clarify as well as confuse our understanding of the disease. We are therefore fortunate in having a noted American scientist, with over forty years of experience in the subject, to criticize and evaluate the existing information. The author does not maintain that his book is a comprehensive survey of all the available literature on the subject; however, it is an excellent compilation of original observations and the more significant investigations of other authorities. In some instances, the author tends to present the literature that substantially gives his own opinions, while excluding contradictory evidence. For instance, he makes the statement that infected food handlers are "undoubtedly the most common and most important method of transmission in many localities." In support of this statement, he cites among others the recent observations of Schoenleber (1940). Although this may well be the case, he has failed to mention the careful study of Sapero and Johnson (1939) that minimized the importance of food handlers as transmitters.

The book is written in the same pleasing style and has the same excellent organization that characterized the earlier monograph. The new publication has also approximately the same illustrations as those in the first, but they are not reproduced quite as well. All of the references are placed at the end of the book, preceding the author and subject indices.

M. M. BROOKE

STITT'S DIAGNOSIS, PREVENTION AND TREATMENT OF TROPICAL DISEASES. *Seventh Edition, in Two Volumes. By Richard P. Strong. The Blakiston Company, Philadelphia.* $21.00. xvii + 871 + xl pp.; vii + 872–1747 + xl pp.; 2 plates. 1944.

Since Strong first revised E. R. Stitt's book in 1942, there have been four reprintings and one new edition. This great demand for the book testifies to the excellence of the presentation and the increasing importance of the subject. In this regard, it is well to note the author's statement in the preface to the 7th edition: "The necessity for a new edition emphasizes anew that under the greatly changed conditions of this world and the increased opportunities for the aerial transportation of disease, the medical profession has come to recognize the growing importance of tropical medicine and its world-wide significance and feels that further knowledge and especially the dissemination of new information regarding it is desirable."

There are many small handbooks of tropical medicine but few publications that thoroughly discuss the various aspects of this broad subject. In addition to the essential facts, a great deal of the original literature is here reviewed, enabling the reader to acquire a much deeper insight into the present status of our knowledge and to become aware of the existing problems that are yet to be solved. The thorough presentation of the subject and the dependability of the information has made it the one publication most frequently recommended to the serious student. The tremendous amount of fascinating data that it records makes it a valuable possession of even the specialist in the various fields it covers.

When one considers the caliber of the men responsible for the existence of this book, there is little wonder why it is generally considered to be the finest American publication on the subject. Its basic organization and subject matter were derived from the original work by Stitt that underwent five careful revisions. When Strong rewrote and enlarged Stitt's book for the sixth and seventh editions, he not only added materially from his own wealth of knowledge, but he also obtained assistance and contributions from such outstanding scientists as Bayne-Jones, Meleney, Sawyer, Komp, Russell, Hakansson, Sellards, Simmons, Callender, Shattuck, Sapero, Sandround, Mackie, and Barbour. In addition to this assistance, Strong gives recognition to the fact that he obtained aid from the authoritative works of Craig and Faust, Chandler, Manson-Bahr, and Stitt, Clough and Clough.

The diseases presented in this two-volume publication are those that "occur commonly or most frequently in tropical countries." This does not mean, however, that these diseases occur only in the tropics, for "many of them are encountered from time to time and some are even endemic in countries with temperate climates." For this reason, the whole gamut of etiological agents is considered—protozoa, bacteria, viruses, rickettsiae, fungi, and helminthes. In addition, considerable attention is given to the poisonous plants, fish, snakes, and coelenterates, and to injurious arthropods. Toward the end of the book the author has presented helpful discussions on problems of personal hygiene and medical practice in the tropics.

The appendix includes information on disinfectants and disinfestants, and alphabetically arranged notes on clinical diagnosis and laboratory procedures. Rather extensive lists of references are to be found at the end of each chapter. Unlike some of the other publications that consist of two volumes, the complete

table of contents and index are repeated in each volume for the convenience of the reader.

M. M. BROOKE

ON MODERN SYPHILOTHERAPY *With Particular Reference to Salvarsan.*

By Albert Neisser; translated by Isabelle von Sazenhofen Wartenberg. The Johns Hopkins Press, Baltimore. $1.00. 42 pp.; 1 plate. 1945.

This reprinted essay of 1911 is one of the first based on reasonably extensive use of Ehrlich's famous '606.' It gives an excellent picture of the early methods of arsphenamine treatment, and contains many ideas of a most experienced clinician that are still of value for practising syphilologists. Its lucid style makes it very good reading. Francis Tomlinson Gardner's bibliography and delightful biographical sketch of the great German scientist from Breslau, the memory of whom the Nazis have tried in vain to obliterate, add considerably to the value of the excellent little volume.

ERWIN H. ACKERKNECHT

THE ABORTION PROBLEM. *Proceedings of the Conference Held Under the Auspices of the National Committee on Maternal Health, Inc., at the New York Academy of Medicine, June 19th and 20th, 1942.*

Howard C. Taylor, Jr., Conference Chairman. Published for the National Committee on Maternal Health Inc., by The Williams Wilkins & Company, Baltimore. $3.50. xii + 182 pp. 1944.

Except for three papers which were omitted, either at the request of the author or for the purpose of earlier publication, this volume represents the Proceedings of the Conference on Abortion Problems, sponsored by the National Committee on Maternal Health, which was held in New York City on June 19 and 20, 1942. Some 50 prominent members of the medical, legal, and teaching professions were in attendance for the purpose of discussing the general problem of human abortion under the following headings: (1) The Magnitude of the Abortion Problem; (2) Spontaneous Abortion and its Prevention; (3) Social, Moral, and Economic Causes and Control of Abortion; and (4) The Control of the Abortion Problem.

The discussions relative to the magnitude of the abortion problem were unanimous in their consensus that it is truly enormous in America. This can be more easily grasped when it is realized that some 30 per cent of all maternal mortality follows abortion.

The problem of spontaneous abortion as related to embryology, endocrinology, and serology is apparently being handled very satisfactorily as a result of fruitful physiological research. The social, legal, and medical aspects of criminal abortion are extremely complicated and difficult to analyse; hence, the utter failure to date in attempting to curb this dangerous, yet lucrative practice. Basic educational and religious training concerning the value of human life and the use of contraceptives, as well as the social stigma attached to the phenomenon of giving birth to an illegitimate child, are all tremendous forces acting either for or against the inception and subsequent attempt to terminate an unwanted pregnancy.

In the matter of controlling criminal abortions, a number of cogent suggestions have been presented, namely: (1) better legislation, not only for the purpose of outlawing the criminal abortionist, but also for protecting the honest practitioner in cases where surgical abortion is indicated; (2) improved group medical insurance, to cover not only the hospitalization of maternity cases, but pre- and post-natal care as well; (3) a system of pregnancy registration and public health control, just as in the case of epidemic or contagious diseases; (4) a complete system of education in the physiology of sex and reproduction, the use of contraceptives, and the dangers of abortion, to be carried out in the public schools, pre-marital clinics, hospitals, and general practitioners' offices; and finally (5), since the criminal abortion problem maintains itself almost entirely on public ignorance and official indifference, the fundamental method of bringing it out into the open where it can be made a topic for open discussion and action, similar to the manner in which the syphilis problem was forced upon the public mind some five or six years ago.

These papers are as refreshing as they are enlightening, and will undoubtedly be read with much interest, not only by the medical profession, but by all social, legal, and research workers interested in the problem of human abortion. There is an index to the volume.

B. AUBREY SCHNEIDER

WOMAN'S MEDICAL PROBLEMS.

By Maxine Davis. Whittlesey House, McGraw-Hill Company, New York and London. $2.00. xv + 220 pp. 1945.

While one might assume that the natural adviser on medical problems should be a physician, there are few medical men who could have done as good a job with the subject matter of the book under review as has its author, a laywoman. Besides a natural gift for and a long training in popular writing on health matters, she has obviously familiarized herself very thoroughly with the various topics which she discusses in her book. They are presented soundly and readably, with no suggestion of De Kruiffian sensationalism.

Eighteen chapters make up the volume, dealing with such subjects as menstruation, pregnancy, abortion, childbirth, sterility, cancer, etc. The fact that much of the material has been previously published in Good

Housekeeping magazine does not detract from its value. Miss Davis studiously avoids including any suggestions as to treatment, and even a very critical medical reader will find little to criticize from the standpoint of scientific accuracy. The Foreword is by Dr. Fred Adair.

A few minor flaws may be noted. For example, in discussing uterine bleeding she emphatically states: "Never let a doctor use radium." She can scarcely mean this, for practically all gynecologists consider that one of the most important of all indications for the use of radium or x-ray is in the cure of the very common functional bleedings in women approaching the menopausal epoch. Again, she obviously confuses stem pessaries (for cervical insertion) and the vaginal pessaries used in the management of displacements. In the chapter devoted to Caesarean section she uses the spelling Caesarian throughout, and this is not in conformity with either usage or dictionaries.

Aside from a few minor flaws of this type, however, the book impresses the reviewer as an excellent one, which women can be expected to read with pleasure and profit, and without any of the apprehensions with which the average reader is apt to approach health literature in general.

EMIL NOVAK

THE PATHOGENESIS OF TUBERCULOSIS.

By Arnold R. Rich. Charles C. Thomas, Springfield, Ill. and Baltimore. $10.50. xxiv + 1008 pp. 1944.

Aside from being a monumental storehouse of information on all phases of tuberculosis, this book, containing 21 chapters, should be of great interest to the research worker. The author critically analyzes practically all of the 1417 references, utilizing those portions which substantiate his convictions on the phase under discussion. The forceful technic of expression is stimulating to the reader, who soon finds the book pleasant reading matter and himself infected with a similar analytical attitude of mind.

The purpose of the book has been "to present the background for the interpretation of the pathogenesis of the disease, i.e., the factors that determine the character of the lesions, and the progression or arrest of the infection, and the principles that govern the action of those factors."

In the opening chapter, Rich discusses the three types of chemical constituents, protein, carbohydrate, and lipid, which have been isolated from the tubercle bacillus, and notes that while they are undoubtedly responsible for the occurrence of the tissue damage, constitutional symptoms, formation of epithelioid and giant cells, and thus tubercles, nevertheless amounts of each of the isolated fractions far greater than would exist in natural infection must be injected into normal animals in order to produce these effects. He repeatedly states that no constituent of the tubercle bacillus with toxicity for the normal animal probably exists, and yet he points out that with continued purification of the protein it becomes less toxic, which, of course, implies the removal of a toxic fraction. Furthermore, in a later chapter he states that the acute inflammation produced by injecting tubercle bacilli into the normal body varies directly with the number of bacilli introduced, and that this effect cannot be regarded as a non-specific or traumatic effect, since the injection of a corresponding amount of saline does not give a similar effect. The protein becomes a highly active poison in the sensitized tuberculous animal and therefore responsible for necrosis. It must constitute an essential part of the sensitizing agent.

The relationship of the various types of acid-fast bacilli to their pathogenicity for various animals is considered, and on the basis of existing evidence Rich questions the possibility of infection in man with the avian bacillus. Types of tubercle bacilli are differentiated chiefly on the basis of differences in culture and pathogenicity, and the question of alteration of their virulence and morphology are considered. The true nature of virulence is not clearly understood, but it is dependent upon the survival and multiplication of the bacilli in the body.

In the chapter on native resistance in different species and races, there is a very instructive discussion of the difference between childhood and adult types of tuberculosis and its importance in analyzing the well-known differences in resistance which exist between the negro and white races. The various factors, such as heredity, sex, age, dosage of bacilli, repetition of infection, location of lesion, and environmental factors important in individual resistance are discussed at length. Such factors as temperature, oxygen supply, hydrogen-ion concentration, phagocytosis, etc., are important in causing inhibition of the progressive multiplication of tubercle bacilli in the tissues and thus in determining native resistance.

The author attempts to clear up the misconceptions and confusions as to the various types of hypersensitivity. In the anaphylactic or Arthus and pollen type of hypersensitivity, there is specific sensitization of the involuntary muscle, vascular endothelium, and possibly of the glandular epithelial cells of the respiratory tract; and antibody exists in the blood stream and can be passively transferred. On the other hand, in the bacterial or tuberculin type of sensitivity there is a gradual and progressive local appearance of inflammation, and the cells in general are sensitized. Although the attempts of various investigators to demonstrate the presence of sensitizing antibody in this type of sensitivity have met only with irregular success, Rich says that there are persuasive reasons for believing that there is specific antibody and that it is possible that it may be closely associated with the cells instead of being free in the circulation, where it would be available for passive transfer. Desensitization can be effected in all

these types of hypersensitivity, and the mechanism and implications of desensitization are discussed.

A certain degree of resistance to the tubercle bacillus can be acquired by previous infection or by vaccination, but this is dependent upon the number and virulence of the bacilli, the route of infection, and the individual constitution. In discussing the role of hypersensitivity in acquired resistance, Rich concludes that "one should certainly avoid both the view that hypersensitivity is always deleterious, and the view that it is always essential for the successful operation of acquired resistance." While it has not been demonstrated that antibodies play an important role in acquired resistance, it is probable that they do. With acquired resistance there is also a lymphocytosis, and the bacilli do not survive as well in the mononuclear phagocytes. Many practical factors, such as nutrition, physical and mental overstrain, intercurrent infections, trauma, occupation, endocrine disturbances, alcoholism, and climate have an important influence on resistance. Dosage of tubercle bacilli and frequency of infection also are important. Rich does not find the available evidence sufficient to believe that an occupational hazard exists for doctors, medical students, or nurses.

The factors concerned in the formation of epithelioid cells, giant cells, tubercles, inflammation, necrosis, caseation and softening, the symptoms in tuberculosis, and those factors responsible for the differences in children and adults, as well as the reason for the apical site of adult type pulmonary tuberculosis are considered. Both endogenous and exogenous reinfection occur, but it still is not known which is more common. Familiar types of lesions are presented and there are many beautiful illustrations of the pathological effects of tuberculosis throughout the book.

The concluding chapter shows that the mortality rate from tuberculosis has decreased phenomenally during the past 40 years, so that it now holds seventh instead of first place in the list of leading causes of death in the United States. Nevertheless, it is still the most common cause of death in the important age period from 15 to 45. Rich believes the most effective cause for this decline has been the increasing improvement in living conditions.

In such a comprehensive discussion, it is to be regretted that some mention was not made of the changes which occur during the development of tuberculosis in the blood and pleural exudates, such as those affecting the serum proteins, cholesterol, lecithin, and sedimentation. A discussion of amyloidosis would also be welcome.

Some typographical errors exist, but on the whole the book is well published. It is a necessary addition to the library of physician, research worker, and student.

FLORENCE B. SEIBERT

PENICILLIN THERAPY *including Tyrothricin and other Antibiotic Therapy.*

By John A. Kolmer. D. Appleton-Century Company, New York and London. $5.00. xv + 302 pp. 1945.

The very large literature that has accumulated on the methods of standardization, the properties, dosage, and forms of administration of penicillin makes it desirable to have such a concise text as the present volume for the medical practitioner. The author has succeeded in reviewing and summarizing the important literature on this subject, although no attempt has been made to include all published articles. The medical teacher and biologist will find also in this small volume all the information he may desire concerning the clinical applications of penicillin and tyrothricin.

A general review of the antibiotic drugs is presented. Penicillin production methods and assay technique are described in detail, so that an experienced hospital bacteriologist could conduct assays by following the directions set forth in the text. In accordance with the title, the major portion of the volume is devoted to details of penicillin administration in the clinic. The author has maintained a judicial sense in reviewing the clinical literature, and he has not hesitated to point out over-enthusiastic reports. A complete bibliography is given with each of the book's twenty-three chapters.

The usual intramuscular methods of administration of penicillin are described completely. The increasingly popular bone-marrow, or intramedullary type of injection is described, and techniques are diagrammed. New oral administration methods are set forth in the appendix. The clinician will find much of value here.

C. J. CARR

YELLOW MAGIC. *The Story of Penicillin.*

By J. D. Ratcliff. Random House, New York. $2.00. xv + 173 pp. 1945.

From its beginning—"I have no doubt that many people will quarrel with the title of this book"—to its last words, those of Fleming himself: "People have called penicillin a miracle. For once in my life as a scientist, I agree. It is a miracle which will save lives by the thousand"—this book is good reading, a worthy example of popular science at its best. Not only the early work of Fleming on *Penicillium notatum* and the labors of Florey, Chain, and their coworkers here find a fitting memorial. The opening of the entire field of chemotherapy through the work of Ehrlich, the later herculean efforts of Domagk ending in the discovery of prontosil and the sulfa drugs, and the pioneering efforts of Dubos of the Rockefeller Institute to find and extract a germ-killer from soil bacteria are also recounted and place the current story of penicillin in its appropriate setting. The story of mass *Penicillium* culture and penicillin production in America during the war is

dramatic. The story of the clinical testing of penicillin in cases of wounds, burns, and gas gangrene, staph infections, meningitis, gonorrhea, osteomyelitis, bacterial endocarditis and syphilis and other diseases cannot fail to stir the spirit. Only once in a great while does the narrative run away from the author, as when he talks of "colonies" of pneumococci in the blood, or colonies of staphs "looking feeble."

In most respects, too, the account seems well balanced and not given to over-exaggeration. Only in regard to the mutability of organisms to resistant forms is too little said. The mutability of *Penicillium* itself is regarded as environmental modification rather than genetic change. Perhaps penicillin's entire non-toxicity is not left sufficiently open to question. These are very minor flaws. By and large, few such admirable books have been written about science for the average layman. The book includes excellent photographs, illustrations, and a useful index, unusual in a book of this sort.

BENTLEY GLASS

THE STORY OF PENICILLIN.

By Boris Sokoloff. Ziff-Davis Publishing Company, Chicago and New York. $2.00. 167 pp. 1945.

The story of penicillin is here told briefly in non-technical style. The first chapter introduces the reader to Metchnikoff and Metalnikoff, and to the accumulation of evidence of antibiosis even in the early days of bacteriology. The discovery of penicillin is then described, and the development and experimentation with the drug in England. Under the title "Cinderella of Science," problems of production are discussed, the role of paraamino benzoic acid is presented and also the therapeutic action of penicillin B and proflavin. The use of penicillin is described mainly by presentation of case histories. The fifth chapter is devoted to other "wonderful tiny chemist-molds" and some of the substances derived from them. The use of moldy bread for wound dressings among European peasants is mentioned. The concluding chapter emphasizes the continuity of ideas in scientific research.

Unfortunately the text does not progress very smoothly. At times the reader is apt to be confused by the author's detours. A more straightforward and sober presentation would be desired by most people. More facts as to why penicillin is a "wonder drug" could have been included. On the subject of Pasteur versus Ehrlich, some scientists might well take issue.

Appended to the text are 13 pages of footnotes, an extensive bibliography for a book of this type, and an index. There are no illustrations.

HARRIETTE VERA

FUNDAMENTALS OF PHARMACOLOGY.

By Clinton H. Thienes. Paul B. Hoeber, Medical Book Department of Harper and Bros., New York and London. $5.75. xviii + 497 pp. 1945.

This book is intended to present the aspects of pharmacology which are essential to a medical student and to refresh the knowledge of the practitioner.

The source, chemical nature, preparation, dose, absorption, distribution, fate, excretion, and actions of each drug as determined experimentally are emphasized.

The material is arranged according to the site of action of the drug (central nervous system, peripheral nervous system, muscles, diuretics); the antiparasitics; the hormones, special minerals, and tissue extracts; vitamins; drugs which act on body surfaces; chemical diagnostic agents; actions of drugs on cells; and pharmacy and prescription writing.

The material is presented in a clear and well organized manner and should be a valuable text for the medical student. Since the scope is limited, it is probable that most biologists would find a general reference book more useful.

WILLIAM E. EVANS, JR.

ALCOHOL HYGIENE. *Vol. 1, No. 1. September–October, 1944.*

Published bi-monthly for The National Committee on Alcohol Hygiene, Inc.; by Alcoholism Publications, Baltimore. Free upon request.

The stated purpose of this bimonthly bulletin of short papers and abstracts by medically or psychiatrically trained workers is "to disseminate scientific information to the public through various *educators* on the subject of alcoholism with the primary view of *educating* individuals and the community about alcoholism . . . and the significance of this medical psychiatric problem in its effects on, and relation to, both the individual and the community."

PSYCHOLOGY AND ANIMAL BEHAVIOR

THE PSYCHOLOGY OF WOMEN: A PSYCHOANALYTIC INTERPRETATION. *Volume Two—Motherhood.*

By Helene Deutsch. Grune and Stratton, New York. $5.00. vi + 498 pp. 1945.

In this second volume, the author draws to a conclusion her views concerning the psychological life of normal women in our society. Because the treatment is of the normal woman, only passing use is made of pathological material. The volume is concerned essentially with the psychological and physiological aspects of motherhood in their combination as a psychosomatic

problem. Every aspect of motherhood is discussed, and the author's deep understanding of motherliness and of the strict sexual aspects of motherhood are noteworthy. In dealing with the psychology of the sexual act, she discusses some aspects of sexual intercourse which are not generally understood even by experienced psychiatrists. She discusses the psychogenic influences of sterility and fertility with a sureness which most of us lack. Her contributions in this field are important even if they lack substantial proof. Concerning pregnant women she makes the interesting statement that "to a striking degree most intuitive and introspective women shy away from observing their own psychic processes during pregnancy. One might almost say they are deliberately trying not to observe them. This profound motivated behavior is one of the reasons why we have so little information about the psychic life of pregnant women." In spite of this paucity of material, the author gives a remarkable demonstration of her insight into the psychological life, the aspirations, the fears, the imaginations of pregnant women. She quite rightly concludes that the psychological life during pregnancy depends very greatly on the psychological atmosphere at the time of conception. In the chapter on delivery she makes a rather telling statement, that the use of modern methods to relieve woman of pain with delivery may have distinctly deleterious effects on the mother's psychology. She also observes that death of a child at delivery leaves much less residue of psychological upheaval than does the death of a child later on.

The mother-child relationship is superbly described. There follow chapters on problems of unmarried mothers, adoptive mothers, and stepmothers, showing a very great insight into all three of these special problems. She makes the important point that adoptive mothers have in all principal essentials the same opportunity to achieve psychological consequences from their situation that real mothers have. The principal obstacles confronting adoptive mothers lie in their own psychology. She sums up her reaction to the stepmother problem with the statement, "On the whole it can be said that a good mother is also a good stepmother, and so much of this difficult problem can be left to her maternal feelings." A final chapter on the climacterium is notable for the author's insistence that in the climacterium there is a resurgence of the same problems which had first appeared in adolescence. She goes so far as to say that knowing the problems of adolescence in any patient gives her a feeling of safety in prognosticating the course the climacterium will take.

Helene Deutsch would certainly merit inclusion in that group of well-rounded individuals whom the Germans used to call "Menschenkenner," and which I am assured by a psychoanalytic colleague is practically extinct at this time. I am unaware of any other book that begins to cover the subject of Helene Deutsch's choice in such comprehensive fashion. She has observed and recorded her impressions on every variety of woman in every stage of woman's life, with only one exception as far as I can note. She makes no reference to that growing list of women who have achieved motherhood by artificial insemination. Probably she has also observations on these women, but not enough as yet to justify inclusion in a book. She touches upon dramatic portrayals of feminine psychology from belles lettres in this second volume in a fashion much less annoying to this reviewer than in the first volume. Here it is but a casual touch, whereas in the first volume the material seemed to be used for the demonstration of psychological theories in a way that does violence to scientific method.

It is impossible to do justice to such a pair of volumes in any review. One has to read to believe the wealth of observation and understanding, and the helpfulness even of those theories which may seem from time to time to outstrip established fact. This is the second of two volumes which must not be missed by any psychiatrist. All will look forward eagerly to the two additional promised volumes, one on the pathologic manifestations of the psychology of women, and the other on the contribution of cultural and social factors to feminine psychology.

WENDELL MUNCIE

THE HUMAN MIND. *Third Edition, corrected, enlarged, and rewritten.*

By Karl A. Menninger. Alfred A. Knopf, New York. $5.00. xvii + 517 pp. 1945.

To the fourth edition of this excellent introduction to the principles of psychiatry there have been added a section on new developments in therapy and a comprehensive bibliography, arranged by topics corresponding to the organization of the text. The discussion of new therapeutic methods is relatively brief, and there is still only a bare mention of the advances in the knowledge and treatment of military psychiatry made during World War II.

The author's position on the significance of hereditary factors in the determination of neuropathic constitution and mental disease will not satisfy geneticists. He not only seems to follow the general tendency of medical men to think of heredity solely in terms of an unbroken transmission of traits from generation to generation, but quotes with evident approval such nonsense as the statement that "we can *define* environmental forces as those over which we have potential control, and hereditary forces as those over which we have as yet no possibility of control." This means, no doubt, that because polydactyly can be corrected by surgery, and diabetes mellitus by insulin administration, they thereby cease to be hereditary. He also speaks scathingly of the interpretations of studies of twins. It is certainly true that identical twins as a

rule create for themselves a somewhat more similar environment than even same-sexed fraternal twins, but that is hardly sufficient ground for denying any validity to the conclusions as to the significance of genetic similarity. It is noteworthy that the work of Rosanoff in this field is completely ignored.

Biologists who wish to settle in their own minds the question of the balance in modern psychiatry between science, ars medica, psychoanalytic dogma, and unsupported speculation can hardly find a better book for their purpose, or a more authoritative exponent than the author.

BENTLEY GLASS

INVISIBLE ANATOMY. *A Study of 'Nerves', Hysteria and Sex.*

By E. Graham Howe. Faber and Faber, London. 10s. 6d. 333 pp. 1944.

This is a treatment of personality from various aspects, normal and abnormal, following Jungian analytical psychology. The author resorts to diagrams to bring illumination into his thought concerning the structure and workings of personality. They may do just that for him but they fail to accomplish it for the reviewer. On the other hand, Howe's special chapters dealing with various psychopathology pictures, neuroses, sex abnormalities, and peculiar characters of other sorts are masterfully written, and the author has evidently in some cases achieved considerable therapeutic success by the ingenious use of analytical psychological methods. As with any psychiatric presentation, there is a great deal of fundamental and worth-while observation, and as with most such presentations there is a heavy overloading with theory, speculation, and mythology put to the use of theory. Too much importance is attached to all this theoretical speculation, and the actual facts of this small book could be expressed in half the space with no deficit in the reader's understanding.

WENDELL MUNCIE

THE LIFE OF CHILDHOOD. *A Contribution to Analytical Psychology.*

By Michael Fordham. Kegan Paul, Trench, Trubner and Co., London. 15s. viii + 154 pp. 1944.

This book provides an extension of Jung's teachings of analytical psychology to the realm of normal and problem children. The principal differentiation of this material from other varieties of child psychology is the introduction of Jungian archetypes as the symbols necessary to the proper understanding of child psychology. Actually, the observations of children as set forth in this book show nothing different from the observations made by other methods, and the author's painstaking analysis of child problems and his evident therapeutic successes with them point to the usefulness of his type of treatment, besides the other types of treatment, too. An interesting part of the book has to do with children's drawings, and here also the archetypes are introduced as explanation in a manner which leaves me unconvinced, but the ingenuity of the process must be acknowledged. The last chapter on treatment is full of sage advice, sage whether one is a Jungian or an adherent of any other system.

WENDELL MUNCIE

PSYCHOLOGY FOR THE RETURNING SERVICEMAN.

Prepared by a Committee of the National Research Council. Edited by Irvin L. Child and Marjorie Van De Water. Infantry Journal, Washington; Penguin Books, New York. 25 cents (paper). 243 pp. 1945.

The serviceman returning from World War II is faced with a multitude of physical, mental, and social problems and adjustments of such magnitude that the services now recognize, as part of their duty, the necessity for a complete program of general rehabilitation and personnel guidance. This little volume has been prepared by a number of Army and Navy personnel, civilian physicians, psychologists, psychiatrists, and general workers in the fields of recreation, sociology, welfare, and guidance, and is intended primarily for use by the returning serviceman and his family in bridging the gap between his life on the fighting front and life back home.

Although it is apparent that many servicemen have withstood the stresses of regimentation and combat without any noticeable changes in personality or outlook on life, there are others whose experiences in the front lines or in prison camps have left deep-seated mental impressions or serious physical handicaps which interfere with a normal return to life and work at home. In many instances the difficulties are of such a subtle nature that the man is not able to recognize them for himself; hence the need for consulting a psychologist or psychiatrist. In other instances where the difficulties are imposed by the loss of limbs, sight, hearing, etc., the problem of adjustment must be accomplished by the process of learning new skills and techniques in order to begin an entirely new and different type of life than was contemplated before the war.

An enormous amount of sound reason and common sense has been instilled into these eighteen chapters concerning such pertinent subjects as meeting people, finding a job, picking a wife, returning to a family, recovering from mental or physical illness or injury, and in general, taking part in a normal society again.

Although some servicemen will undoubtedly resent the thought that they are considered different, and that they need a little book to help them get adjusted,

there are others who will welcome the kindly and helpful words of encouragement and advice offered in these pages.

B. AUBREY SCHNEIDER

TEXTBOOK OF ABNORMAL PSYCHOLOGY. *Third Edition. By Roy M. Dorcus and G. Wilson Shaffer. The Williams & Wilkins Company, Baltimore.* $4.00. xv + 547 pp. 1945.

In this third edition of the authors' well known work on abnormal psychology considerable new material has been added. Recent published research has added some 365 new references. The book follows the form contained in the two previous editions, with chapters on: the field and scope of abnormal psychology; sensory disorders; motor disorders; disorders of association and memory; theories of disorders of the central functions; desires, feelings, and emotions; sleep, dreams, and hypnosis; classification of mental diseases—organic psychoses and epilepsy, functional psychoses, psychoneuroses, mental deficiency and psychopathic personality; treatment by physical and chemical assault; and psychotherapy. There is a bibliography and an index.

The authors give a very well balanced presentation, from both theoretical and practical standpoints, of the current thought concerning abnormal psychology and the treatment of abnormal psychological conditions. There is an absence of any particular bias and also a healthy lack of that kind of enthusiasm for any special method which so often leads to scientific fallacy.

Just what the difference between this text and a textbook of psychiatry may be, it is hard to see. The same material is covered in the ordinary textbook of psychiatry, and it may be that Ross McC. Chapman's foreword calling attention to the fact that he still thinks that it is important for treatment to be in the hands of medically trained personnel constitutes a necessary warning to those psychologists who might from acquaintance with this material presuppose a capacity to handle psychiatric material independent of a physician. This last is clearly not the stand taken by the present authors, however, for they make it plain that a psychologist should never attempt treatment of the patient without the benefit of complete physical examination. It is doubtful whether this is enough, however, because the whole problem of psychosomatic medicine, the effects of emotions on physical functioning, and the revision of this situation presuppose in the treating authority an intimate acquaintanceship with material that is not at the fingertips of psychologists. It may be that close cooperation would accomplish the same ends, and no doubt has accomplished the same ends in plenty of cases.

This is an excellent textbook, showing a grasp of every aspect of the field, bolstered by a great deal of material of a bibliographical sort, and yet so presented as to be quite easily readable. One very desirable feature in the text is the constant endeavor to present the principles of abnormal psychology through the consideration of the normal. The text will be found very useful to students of psychology as well as to medical students, and even to psychiatrists and psychologists with long experience in their fields.

WENDELL MUNCIE

CRIME AND THE HUMAN MIND.

By David Abrahamsen. Columbia University Press, New York. $3.00. xii + 244 pp. 1944.

This book from the hand of a well-known Norwegian criminologist, now working in this country, gives an excellent statement of present views concerning that subject, particularly concerning the psychological and psychiatric aspects of crime. The theses are illustrated by case histories from the author's own records and from other sources. He seems too optimistic in concluding that psychoanalysis can make any sizable change in certain psychopathic states. This seems to be his stock recommendation. Yet the book is an excellent statement of the general subject and the present status of criminology.

WENDELL MUNCIE

HUMAN BIOLOGY

THE ALEUTIAN AND COMMANDER ISLANDS AND THEIR INHABITANTS.

By Aleš Hrdlička. The Wistar Institute of Anatomy and Biology, Philadelphia. $5.00. xx + 630 pp. 1945.

In this posthumous publication the author has recorded his experiences and anthropological work during three summers (1936 to 1938) on the chain of islands bordering the Behring Sea. The introductory section of this monograph presents brief general and historical notes in regard to the Aleutian Islands. In a large second part (186 pages), all available ethnographical data on the Aleuts have been compiled and abstracted from widely scattered literature. This orderly collection of more or less fragmentary information preserves a picture, though quite incomplete, of the customs, clothes, food, habits, etc., of the Aleutian natives as found by earlier explorers.

The third part (186 pages) describes the author's trips and surveys in the islands, recounting the daily happenings on the basis of field-diaries. These simple notes supply random information on the weather, transportation, food, etc., on these barren islands and enumerate the findings of the remains of the aboriginals and their work in the ground or in caves. This

section includes much irrelevant, repetitious, and quite personal detail which does not fit too well in the frame of an otherwise technical, scholarly volume.

A fourth part (83 pages) is devoted to the archeological objects from the Aleutian Islands, placing them on record in a purely descriptive manner, and without going into comparative aspects. In the final, fifth part (98 pages) the human skeletal remains, successfully collected in adequate series, are thoroughly catalogued with the most essential measurements and systematically compared with corresponding data for other human groups.

These interesting discoveries and analyses are summarized in part as follows:

"All the habitable islands in the Aleutian archipelago, barring the Commanders, had once been occupied by man; and man had multiplied there until he had to utilize every spot fit for human habitation.... The skeletal remains recovered from all older sites, and to some extent also the cultural objects, have shown clearly that the Aleutians had in the course of time been occupied by two physically and also otherwise distinct strains of people, the Aleut and before them the 'pre-Aleut'....There was found not the slightest indication that there might have been even older inhabitants of the islands than the pre-Aleuts. The advent of the latter may, it seems, be referred to the earlier part of the Christian era, but not before this. The coming of the Aleuts, which according to many indications was a gradual spread from the Peninsula westward, could not have taken place more than a few centuries before the coming of the Russians. They met the older population in the islands, replaced it, probably by force, in the more eastern parts of the archipelago, and then spread thinly westward, admixing there with what remained of the older people. The Commander Islands, it is now positive, were never peopled before the advent of the Russians."

"Somatically, the pre-Aleuts differed very considerably from the Aleuts. They were larger people, oblong-headed, and with various other skeletal differences....Neither the pre-Aleut nor the Aleut were Eskimo, or even nearly related physically to the latter. In their cranial characters the pre-Aleuts resembled closely the Siouan Indian, while the Aleuts were very close to one type of the Siberian Tungus."

With these ably supported conclusions this monograph contributes a significant chapter to the prehistory of the New World.

The volume ends with three short appendices (two dealing with ethnographical matters and one with associated animal remains), with a valuable bibliography, and an all-too-condensed index. This monograph has been printed by the Wistar Institute in a perfect fashion, including the reproduction of the 239 illustrations.

A. H. SCHULTZ

A SUMMARY OF YUKI CULTURE. *Anthropological Records, Volume 5, No. 3.*

By George M. Foster. University of California Press, Berkeley and Los Angeles. $1.00 (paper). Pp. iv + 155–244 + 1 map. 1944.

Basing his treatment on information obtained in 1937 from the few survivors of the Yuki, a North Central Californian tribe, the author has tried to make as full a reconstruction as possible of the old material and spiritual culture. The material culture of these warlike hunters was poor; their religion is remarkable in several respects, particularly through the belief in a High God. Of special interest in this valuable little monograph are the materials on Yuki medicine and on religion, including the Pentecostal movement. The absence of any information on the forms of shelter is surprising.

ERWIN H. ACKERKNECHT

ONE AMERICA: *The History, Contributions, and Present Problems of Our Racial and National Minorities. Revised Edition.*

Edited by Francis J. Brown and Joseph Slabey Roucek. Prentice-Hall, New York. $3.75. xvi + 717 pp. 1945.

This is a thoroughly revised edition of the earlier book, *Our Racial and National Minorities*. Thirty-five contributors endeavor to give a short historical view of the forty-three "minorities" of which this nation is composed, of their organizations, press, and cultural contributions. While the organizational data provided are in general adequate, the treatment of the cultural contributions is often much less satisfactory. A second part of the book consists of twenty-three sometimes excellent general essays on the foreign language and negro press and radio broadcasts, on national minorities in foreign and domestic politics, on the education and religion of the foreign born, etc. A final group of essays undertakes to develop a program of cultural democracy in the U. S. The volume contains no contribution whatsoever that deals with the many biological and pseudobiological aspects of the minority problem.

The intentions of the book are most commendable. A scholarly level is in general maintained (though to find, e.g., on p. 97 of a *revised* edition the Sieur Cavalier de LaSalle and the trader Joliet characterized as "fathers" and "missionaries" is somewhat painful). The book includes an enormous amount of valuable information in a relatively limited space for ready reference. In an extensive bibliography the interested student will find an excellent tool for further research upon special problems.

ERWIN H. ACKERKNECHT

ANGEL IN THE FOREST. *A Fairy Tale of Two Utopias. By Margaret Young. Reynal and Hitchcock, New York.* $3.00. 313 pp. 1945.

The story of New Harmony has been told many times, but it bears retelling. Such a significant chapter in the history of the development of American scientific thought deserves a better fate than the neglect which has too often been its fate; consequently every new contribution is to be welcomed. But the work here under review must be considered a failure, in so far as contributing anything new is concerned.

This book is hard to classify. It is not factual history, whose function is to tell the truth and nothing but the truth; nor is it philosophical history, whose function is to interpret the truth; nor is it epic history, whose function is to allegorize the truth. According to the blurb it is "history-fable," and the function of history-fable, if one is to judge by this example, seems to be to do violence to the truth. Consider, for instance, the brief biographical sketch of Charles Alexander LeSueur.

According to the author, LeSueur was a member of the ill-starred expedition led by La Perouse, and escaped the disaster which overtook that expedition by remaining at Botany Bay to collect natural history material. After weeks, months, and finally years had passed, LeSueur was rescued very much against his will by the crew of an English vessel, who forcibly abducted him, only to put him ashore at the first port of call when they learned who he was. This happened to be in the West Indies, where he met William Maclure, who was recruiting settlers for Robert Owen's cooperative colony at New Harmony.

But La Perouse was wrecked in 1788, and New Harmony was founded in 1824. To the reviewer it does not seem reasonable that LeSueur should have waited all that time for the return of his companions without having any suspicion of the disaster that overtook them, nor that the first port of call of a vessel leaving Australia should have been in the West Indies. As a matter of fact, however, LeSueur was only seven years old when La Perouse sailed from France, and it is inconceivable that he could have accompanied the expedition. Furthermore, his name does not appear on the roster of personnel which was among the documents that La Perouse sent back to Paris from Petropavlovsk by De Lesseps. Later, in 1804, LeSueur did go to Australia with the Baudin expedition, as an artist and not as a scientist; but in 1805 he was back in France, having returned safely with Baudin. In 1809 he was exploring the surface waters of the Mediterranean with his towing net, and in 1815 he was in Philadelphia publishing the results of this investigation. That Maclure met LeSueur while recruiting for Owen is extremely unlikely; for Maclure did not become acquainted with Owen until 1824, when he had already been president of the Academy of Natural Sciences for seven years, an organization of which LeSueur was a staff member.

This is perhaps the most flagrant historical inaccuracy in the book, but there are many similar ones of a minor nature; for example, the misspelling of such a well-known name as that of Rafinesque. Another is the statement that Maclure died in Mexico City. The Mexican capital, like our own, is not in the state of the same name, but in the Federal District. Maclure died in the state (but not in the city) of Mexico. The author states that Madame Fretageot was with him when he died, that she died a few days later, that both obituaries were published in the same issue of the New Harmony Disseminator, and that Say died later in the same year. But Weiss and Ziegler, in their biography of Say, put his death in 1834, and those of Madame Fretageot and Maclure in 1833 and 1840, respectively. Further comment appears unnecessary.

The author's statement that celibacy was one of the religious tenets of the Rappites is also open to question. The Rappites were celibates, it is true, but on economic rather than religious grounds. Probably every persecuted minority in eighteenth century Germany was represented among them, but none were required to give up their religious beliefs. They were asked only to be tolerant of each other. There was no unity among them except the unity of mutual respect. It stands to reason that no woman can be an efficient agricultural laborer while carrying a child, and in a community composed of recruits from a nation where it was an ancient tradition that the heavy labor in the field be performed by women, it was not unnatural that they should rebel against being required to function as breeding machines as well. When it became evident that the influx of new members that were arriving from the fatherland on every Ohio river steamer would more than suffice to maintain the population, Rapp began to urge the practice of celibacy, but he continued to perform marriage ceremonies both within and without the colony. On one or two occasions he rebuked parents who brought more children into the world than they could care for, but as he did not discipline the offenders, the statement made by the author that he murdered his own son because the latter's wife was about to bear him a child seems highly improbable.

Throughout the work the author takes a morbid delight in repeating scandal that cannot be verified. The story of the death of young Rapp is only one instance of this. Anothr is the statement that the corpse of a red-haired woman was found in the coffin of Thomas Say when it was removed from the cemetery to the Maclure family vault. The author admits that this story cannot be authenticated. Why then was it repeated? Again, she states that the Quakers sent a shipload of maize to Ireland to alleviate the famine of the 1840's, which failed of this purpose because the Irish didn't know how to prepare corn for eating. It is well known, however, that the Quaker relief ship was laden, not with corn, but with potatoes, with which the Irish were indeed familiar.

The author also tells us that Robert Dale Owen in his old age became both a pacifist and a drunkard. Whether he ever became either one the reviewer is unable to state, but it is inconceivable that he could have become both, for pacifism and inebriety hardly go together. Strength of character is a necessary prerequisite for the former, while the latter is the result of weakness of character. The author's treatment of the letter in which Robert Dale Owen urged Abraham Lincoln to sign the emancipation proclamation is very unsatisfactory. She quotes first Owen, "an imperishable name . . . is within your reach," and then Lincoln, "it thrilled me like a trumpet call." The implication is that the appeal to Lincoln's vanity caused him to sign for his own personal aggrandizement. But Owen's letter was actually a carefully phrased document of several pages, the kind of letter that no president except Lincoln would have had the patience to read, and he explained specifically to Stanton that his desk was covered with letters on the subject, but that those who opposed emancipation did so to safeguard the funds which they had invested in enterprises employing slave labor, while those who favored it did so because they had interests in competing concerns. Robert Dale Owen alone was disinterested, and Lincoln was thrilled because there was at least one person in addition to himself who looked on emancipation not as a matter of expediency, but as one of right and wrong. The author's statements are quite correct, but by picking out a pair of brief and disconnected texts and placing them in juxtaposition she has succeeded in creating a false impression. Also, her comparison of the angel Moroni with an octopus is not only scientifically false but is so nauseating in its coarseness and vulgarity that the reviewer refrains from quoting it.

While speaking of angels, perhaps a few words about Gabriel, who suggested the title for the book, may not be amiss. The archangel is said to have descended on New Harmony to give instructions to Rapp, but no one saw Gabriel but Rapp himself, who produced as evidence the stone on which the archangel stood and which bore the impressions of his feet. The author admits that the stone shows signs of recent recutting. If this is the case, why is it not possible that the entire story dates from a later day than that of the Rappite ascendency? This is the theory advanced by John S. Duss, but he was a Rappite himself and therefore prejudiced. However, if the story really was the fabrication of George Rapp, it seems very strange that he should have left the stone behind when he removed the chosen people from Harmony to Economy. The fact that he took a collection of minerals and other natural curiosities with him, which were subsequently deposited in the museum of the University of Pittsburgh, makes it all the more improbable that he should have left the angel's footprints behind.

One naturally wonders what the author's object may have been in producing a book that departs so far from historical accuracy and common decency. The reviewer can throw no light on this question. That historical accuracy was not her aim is evidenced by the absence of any kind of documentation, bibliography, or index. She is a poet, and her attitude to history is essentially that of an operatic librettist toward his subject. But the reviewer, who has long cherished the ambition some day to visit New Harmony himself, to stand beside the monument erected by Alexander Maclure in memory of Thomas Say, and to see the golden rain trees in bloom, wishes that Marguerite Young could have found some other vehicle for the display of her artistic talent.

ADOLESCENTS IN WARTIME. *The Annals, Volume 236, November 1944.*

Edited by James H. S. Bossard and Eleanor S. Boll. American Academy of Political and Social Science, Philadelphia. $2.50 (cloth), $2.00 (paper); to members $1.50 (cloth), $1.00 (paper). vii + 233 pp. 1944.

This number of the Annals comprises some twenty publications centering around the following general headings: background; social and family setting; wartime employment; health and hygiene; and selected problems. These articles are uniformly good, stimulating, provocative, and thoughtful. Because of this, singling out any special article can only reflect the reviewer's interests. From this standpoint James S. Plant's "Social Significance of War Impact on Adolescents," "Family Backgrounds of Wartime Adolescents" by James H. S. Bossard, "Mental Hygiene Problems of the Adolescent Period" by Dorothy Hankins, "Wartime Recreation for Adolescents" by Roy Sorenson, "Youth and Religion" by Paul Weaver, "Youth and Government" by Katherine F. Lenroot are most welcome, but the whole volume is an important contribution toward understanding this very difficult period in the lives of adolescents. The volume contains also numerous book reviews, seriously and studiously well done.

WENDELL MUNCIE

DELINQUENCY AND THE COMMUNITY IN WARTIME. *1943 Yearbook, National Probation Association.*

Edited by Marjorie Bell. National Probation Association, New York. $1.75 (cloth); $1.25 (paper). 307 pp. 1944.

These papers presented at the annual and other meetings of the association deal with delinquency in wartime. The important sections deal with: crime and the community; wartime changes in parole and probation; the federal wartime protective program; the war and juvenile delinquency; delinquency prevention

movements; community care of delinquent children; psychiatric studies, juvenile and adult; and legal digest.

Donald R. Taft's "American Culture and the Treatment of the Offender," Whitcomb H. Allen's "Young Camp Followers," and Basil L. Q. Henriques' "The Wartime Delinquent in England" are especially interesting. The papers are uniformly good.

WENDELL MUNCIE

COOPERATION IN CRIME CONTROL. *1944 Yearbook, National Probation Association.*

Edited by Marjorie Bell. National Probation Association, New York. $1.75 (cloth); $1.25 (paper). 320 pp. 1945.

This is an excellent compilation of papers given at the annual meeting and other meetings of the association, dealing with juvenile delinquency especially in wartime. There are seven important parts: the juvenile court and its community relationships; understanding the delinquent; protection and preventive services; community responsibility for the wartime delinquent; parole and the institution; special problems of the adult offender; legal digest.

Dean Roscoe Pound's "The Juvenile Court and the Law" sets the tone for the volume. It is a scholarly treatise on the position of the juvenile court in actual legal practice. Judge Alexander's "Speaking as One Judge to Another" is a frank statement of the vicissitudes of the juvenile court judge. The articles are uniformly good, provocative, and helpful.

WENDELL MUNCIE

THE DYNAMICS OF CULTURE CHANGE. *An Inquiry into Race Relations in Africa.*

By Bronislaw Malinowski. Edited by Phyllis M. Kaberry. Yale University Press, New Haven; Humphrey Milford, Oxford University Press, London. $2.50. xiv + 171 pp. 1945

This is the third posthumous book of the late Bronislaw Malinowski. It is to be hoped that he will not outdo the record of his English colleague, W. H. R. Rivers, whose executor, G. E. Smith, brought out six volumes of material which Rivers had preferred not to publish during his lifetime.

In the *Dynamics of Culture Change*, Phyllis M. Kaberry has collected from Malinowski's old papers and manuscripts his opinions on a problem which by American anthropologists is generally called acculturation. Malinowski took his practical examples from British possessions in Africa, where he had studied the question. The first part of the book deals with theoretical and methodological aspects. Excellent remarks on what is essential in such studies are coupled with quite superfluous onslaughts upon the historical approach, marked by the other less agreeable characteristics of Malinowski's theoretical writings—his inconsistency, repetitiousness, and somewhat adolescent eagerness to sell his great method. The second, and more original part of the book deals with special problems such as witchcraft, diet, land tenure, and indirect rule among the African natives subject to rapid acculturation.

The shortcomings of the book are partly conditioned by objective factors. The author makes the commendable attempt to implement scientifically a compromise in a situation which, even in the most tactful description, is somewhat embarrassing.

Malinowski, with all his faults, was probably the greatest anthropologist of his generation, and even this posthumous book is therefore quite readable. Yet in my opinion a better contribution of Malinowski to the problems of acculturation and applied anthropology has been made through his numerous pupils who have done quite outstanding work in this special field.

ERWIN H. ACKERKNECHT

A COMPARATIVE DICTIONARY OF THE TAHITIAN LANGUAGE. *Tahitian-English with an English-Tahitian Finding List. Special Publication Number Six.*

By Edmund Andrews and Irene D. Andrews. The Chicago Academy of Sciences, Chicago. $5.00 (paper). xvi + 253 pp. 1944.

DE OMNIBUS REBUS ET QUIBUSDEM ALIIS

THE AUTOBIOGRAPHY OF SCIENCE.

Edited by Forest Ray Moulton and Justus J. Schifferes. Doubleday, Doran and Co., Garden City, New York. $4.00. xxxi + 666 pp. 1945.

This is an anthology of original writing, from the Greeks to modern times, selected as outstanding contributions to the different fields of science.

Such a collection naturally cannot include everything; and perhaps it would be carping to complain that biochemistry does not receive a place, or that among the botanical excerpts no mention is made of the most important of plant processes, photosynthesis. Some of the work of Emil Fischer on proteins, for instance, and Priestley's paper to the Royal Society on the purification of air by plants might perhaps find a place here.

Yet, given the difficulty of selection, it must be granted that the book covers a very wide range and gives an impressive picture of the history of science in an eminently readable form. Not the least advantage of this form is the prominence it often gives to the scientific methods and experiments themselves, which

tend to be buried in the condensed survey that constitutes the more conventional history of science.

EILEEN SUTTON GERSH

SOUTH AMERICA CALLED THEM. *Explorations of the Great Naturalists La Condamine, Humboldt, Darwin, Spruce.*

By Victor Wolfgang von Hagen. Alfred A. Knopf, New York. xii + 311 + ix pp.; 27 plates. 1945.

This is a book for the young fry and for oldsters, too, for those with scientific training and also for those who haven't any. The accounts of the explorations, during a century and a half, of four great naturalists in South America make inspiring and exciting reading. Science and history and human relations are mingled in nice proportions.

There are Charles-Marie de la Condamine and the South American Pedro Maldonado carrying out their triangulations in the high Andes to determine the shape and size of the earth; the Quiteños suspected them of seeking Inca treasure, for otherwise they could only be lunatics. There are Alexander von Humboldt, seeking with Aimé Bonplant for the communication of the waters of the Amazon and Orinoco, and Charles Darwin wandering through Argentina and observing the Fuegians and the Galápagos reptiles. The fourth explorer is Richard Spruce, who for years collected on the Amazon, who watched the rubber boom come to Brazil and who gathered the seeds of the cinchona tree for transportation to India to be grown for quinine, thus transplanting an industry.

SCIENCE YEAR BOOK OF 1945.

Edited by John D. Ratcliff. Doubleday, Doran and Co., Garden City, New York. $2.50. xxviii + 224 pp. 1945.

Like previous volumes of this series, the *Science Year Book of 1945* presents a broad and readable coverage of current science. The introduction is again better than most, if not all, of the articles the editor has chosen from popular journals and magazines. The chemistry of natural gas and its products, and antibiotics in general are here added to piece out the survey. Part I of the book deals with advances in medicine. Part II covers physics and chemistry; Part III, aviation; and Part IV, other sciences. Except for an article on animal migration in the last section, the treatment of subjects of biological interest is thus largely concentrated in the first section. Here one finds no little diversity in quality of presentation, as well as in subject: Penicillin, blood fractionation, the study of pain, the use of tantalum in surgery, corneal transplants, the liver, the skin, children's diseases, mental disease, and fatigue. Two articles on DDT and one on the use of the electronic tube in medicine, from the following section, add to the variety. Most biologists will find this light volume pleasant and informative reading, particularly so in covering those fields of science less familiar to them.

BENTLEY GLASS

THE QUEST OF AMERICAN LIFE. *University of Colorado Studies Series B. Studies in the Humanities, Volume 2, Number 3.*

By George Norlin. University of Colorado, Boulder. 2.00. xvi + 280 pp. 1945.

This work is an interpretation of the lives of some of the more important contributors to liberal thought in the United States. The author calls it a study in humanism, an unfortunate choice of label, since this term has been applied in turn to practically every school of philosophic thought since the renascence of learning, and to some which preceded it. In fact, the author tells us that he is giving the word a new meaning, and seemingly proceeds to apply the label humanist to all those characters he admires, somewhat loosely, regardless of how little they may have in common; otherwise it would be difficult to explain the presence of the gentle and saintly John Woolman in the same category as Theodore Roosevelt, who believed that the end justifies the means, and who carried the big stick.

The aim of the author has been, not to write history, but to interpret personality, and in this he has been eminently successful. His enthusiasm for Roger Williams does not blind him to the fact that Governor John Winthrop and Pastor John Cotton, who were responsible for the expulsion of Williams from Massachusetts, were themselves upright men of good character, who were actuated by the purest motives to do what seemed best for the colony.

Roger Williams has never had a biographer. We do not even know what he looked like. What we know of him comes from a multitude of unrelated sources. He is the most celebrated forgotten man in the history of liberalism in the United States. Yet it was this shadowy figure who laid the foundation on which William Penn erected the Treaty of Shackamaxen, Thomas Jefferson the Declaration of Independence, and Abraham Lincoln the Emancipation Proclamation.

The biographers of Franklin, on the other hand, have been legion, and it is therefore surprising that the author has found so much about him that is new, placing his emphasis on the way in which and the extent to which Franklin's personality was molded by the environment in which he developed. Perhaps this is a truer appraisal of the versatile Philadelphian than most.

In a most interesting chapter, the author rescues Hector St. Jean de Crèvecoeur from undeserved oblivion. The collection of letters which this obscure

Pennsylvania farmer wrote to a friend in England gives us a more authentic and more detailed picture of American frontier life in the eighteenth century than any other. The author has devoted more space to Crèvecoeur than to any other figure in his book, and has quoted from him more extensively. He deserves this attention.

On the whole, the author's treatment of the nineteenth century is not so happy as that of the eighteenth. This is to be expected. We are still too close to Whittier, Emerson, and Whitman to appraise them properly, and so much has been written about Lincoln that it is practically impossible to say anything about him that has not appeared in print elsewhere. But why was Robert G. Ingersoll omitted entirely and Robert M. LaFollette dismissed with one brief sentence?

The final chapter deals with the elder Roosevelt, and it constitutes something of an anti-climax. It is as yet too soon to decide whether Roosevelt the humanitarian or Roosevelt the imperialist will be remembered by posterity.

In such a work as this there are bound to be some minor flaws. But the omission of the part played by the American women in the development of liberalism is indeed a serious defect.

APPROVED LABORATORY TECHNIC. *Clinical Pathological, Bacteriological, Mycological, Virological, Parasitological, Serological, Biochemical and Histological. Fourth Edition.*

By John A. Kolmer and Fred Boerner. D. Appleton-Century Co., New York and London. $10.00. lxii + 1017 pp. 1945.

This edition has been extensively revised and much new material has been added. New sections on the examination of blood, feces, and tissues for animal parasites, on methods for examination of the saliva, pancreas function tests, examinations of the blood and urine for hormones, examination of the blood and urine for vitamins and virological examinations, etc., as well as good illustrations have been added.

Many new methods are included, although none of the approved methods previously described have been omitted. Among the new methods given, to mention only a few, are the newer simplified methods for the cultivation and identification of anaerobic bacteria, methods for detecting the Rh subgroup in relation to blood transfusion, methods for testing the susceptibility of bacteria to penicillin and for the demonstration of penicillin in the blood, exudates, and other body fluids, methods of detecting and identifying the crystals of the sulfonamide compounds in urine, etc.

The bacteriological nomenclature followed in general is out of date. Diphtheria bacilli for example are referred to as *B. diphtheriae*, with the correct or at least now most commonly accepted "Bergy" terminology *Corynebacterium bacterium* given only once and then in parenthesis. This will be very confusing to recent graduates who have been taught the Bergy classification.

The format is new and condensed. The index is good, and the omission of numbered chapters and sections is not confusing. This book certainly ranks very high in its field and is one of the most practical and most widely used reference books in most clinical and public health laboratories. It is very gratifying to know that a Spanish language edition is in preparation.

ELIZABETH PETRAN

THE PACIFIC ISLANDS HANDBOOK 1944. *North American Edition.*

By R. W. Robson. The Macmillan Company, New York. $4.00. xii + 371 pp.; 4 maps. 1945.

This is a surprisingly readable collection of factual material on the Pacific Island world. The readability comes from two sources: the author is a lifelong newspaper man, and he has included much more historical material than it is customary to find in a handbook.

There are many maps of the islands. Data are given on population, government, geography, economics, etc., varying somewhat from island to island, apparently depending upon the amount and type of material available.

If one of the virtues of the book is its enjoyable journalistic style, one must also record that the opening sections, where the war in the Pacific is chronicled, seem too luridly journalistic. Such phrases as "horrible enemy," "our children guarded against the stealthy, creeping treachery of a race so different from our own that it might have come from another planet," seem out of place in scholarly writing.

GEORGE F. CARTER

CARNEGIE INSTITUTION OF WASHINGTON YEAR BOOK NUMBER 43. *July 1, 1943—June 30, 1944, With Administrative Reports through December 15, 1944.*

Carnegie Institution of Washington, Washington, D. C. $1.50 (cloth); $1.00 (paper). xxxiv + 206 pp. 1944.

In addition to the reports of the Executive Committee and of the President of the Carnegie Institution, the Year Book always includes summaries of the research of the Institution's various departments. Biologists have long recognized these reports as having considerable value, since they frequently include preliminary results of researches still in progress, often some years ahead of final publication. Sometimes, too, there are items of special interest that never find their way into other print. The present volume is no exception.

The Division of Plant Biology reports, for example, work on the antibiotic chlorellin and extensive studies on *Poa* and other forage grasses, the latter undertaken to secure, through hybridization and polyploidy, superior types for restocking the Western ranges. New very early human embryos, effects of hormones on the differentiation on the sexual organs of the opossum, and other studies on the sex hormones are reported by the Department of Embryology. There is also a very interesting summary of work on the transfer of substances through the placenta, using heavy water and radioactive sodium as tracers. Report of a sarcoma-producing extract and of a tissue-cultured rhabdomyoma are also worth special notice. The Department of Genetics contributes much the lengthiest report on biological subjects, including: bacteriophage- or ultraviolet-resistant mutants of *E. coli*, penicillin-resistant mutants of *Staph. aureus*, radiation experiments with *Neurospora* and *Penicillium*, and mutations of bacteriophage; extensive studies of the distribution of chromosome breaks in *Drosophila*; mitosis in corneas of rats and rabbits; the genetic structure of natural populations, including studies of frequency changes of chromosome types in artificial populations and of sexual isolation between South and Central American strains of several *Drosophila* species; deficiency mutants and directed mutation in maize; polyploidy in *T. koksaghyz*; the successful breeding of a low-marihuana strain of hemp; a new type of sex chromosome balance in hemp; longevity and foster-nursing in relation to mouse leukemia; and a considerable section embodying the last report on endocrine studies by Oscar Riddle, retiring as investigator this year after a long and productive service with the Carnegie Institution. Perusal of these reports, far better than examination of the financial statement, affords a measure of the great contribution of the Carnegie Institution to American science.

BENTLEY GLASS

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